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*SECTION OF PHYSICS AND CHEMISTRY**

IONIZATION AND DISSOCIATION EQUILIBRIA IN LIQUID SULFUR DIOXIDE SOLUTION: SPHERICAL AND PLANAR IONOPHORES*

By Norman N. Lichtin† and Peter Pappas

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Introduction

Liquid sulfur dioxide is a valuable solvent for studies of the reversible ionization of the covalent carbon to halogen bond: for example, of triphenylmethyl chloride and of related equilibria. However, interpretation of such systems[1-4] is complicated by the smenogenic[5] nature of the solvent. In order to clarify the structural correlation of the equilibria of ionogens,[5] it is necessary to distinguish carefully between structural influences on the ionization and dissociation processes, that is, between the structural dependences of K_1 and K_2 of EQUATION 1.

$$RCl \underset{\longleftarrow}{\xrightarrow{K_1}} \underbrace{R^+Cl^-} \underset{\longleftarrow}{\xrightarrow{K_2}} R^+ + Cl^- \qquad (1)$$

Investigation of the electric conductivity of solutions of a series of ionophores[5] was therefore initiated in order to isolate the process measured by K_2. Initial measurements carried out[6] with KCl, KBr, KI, and Me_4NBr revealed a remarkable degree of adherence to simple sphere in continuum theory of both association behavior and, for the elemental electrolytes, mobility. Thus, Bjerrum distances of closest approach (a_o) calculated[7] from data obtained at two temperatures deviate from sums of crystallographic or van der Waals radii by 0 to 8 per cent, and experimental limiting conductances (Λ_o) of the elemental electrolytes differ by 0 to 10 per cent from those calculated by substitution of ionic radii into Stokes law.[8] The inadequacy of the simple sphere in continuum model for quantitative correlation of electrolyte behavior in other solvents, however, is well established;[9] moreover, a very close similarity among Stokes, Bjerrum, and crystallographic ionic radii is not anticipated even from this type of theory.

Extension of these measurements to a wider variety of ionophores thus becomes of great interest for its own sake, as well as for the value such data have for interpretation of the behavior of ionogens.

Experimental

The apparatus, laboratory procedures, and mathematical treatment of the data were similar to those employed in our earlier work;[4] experimental details are described in the Ph.D. dissertation of Peter Pappas.

*This paper, illustrated with lantern slides, was presented at a meeting of the Section on November 5, 1957.

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The use of Shedlovsky's method[10] of calculating Λ_o and K_{exp} is justified[11] for most, if not all, of the present solutes, since all of the reciprocal association constants (K_{exp}) are smaller than 10^{-2}.

Most solutes were prepared in these laboratories or were commercial materials that were purified before use. Tropenium bromide, C_7H_7Br, was provided by Hyp J. Dauben, Jr., of the University of Washington, Seattle, Wash., to whom we are indebted.

Data

Measurements have been performed on solutions of KBr at five additional temperatures and on solutions of eleven additional ionophores at one or two temperatures. TABLE 1 summarizes results for ionophores composed of ions with spherical or tetrahedral symmetry. TABLE 2 presents data for ionophores in which at least one ion has essentially planar symmetry. The reliability of the data has been estimated[12] by a statistical analysis of the slopes and intercepts of the Shedlovsky lines with the aid of Fisher-t tables and 95 per cent confidence limits. This procedure yields uncertainties about five times as great as the probable errors, and is undoubtedly a conservative measure.

TABLE 1

QUANTITIES DERIVED FROM THE SHEDLOVSKY CALCULATION: SPHERICAL IONOPHORES.

Solute	Temp. °C.	$10^3 K_{exp.}$ Moles/liter	Λ_o mhos cm^2 $mole^{-1}$	(Variance)½ 95 per cent confidence limits (±) Λ_o	$10^3 K_{exp.}$
KBr	−5.25	0.180	233	2	0.007
	−10.71	0.211	224	2	0.010
	−15.56	0.251	212	2	0.012
	−20.58	0.288	202	2	0.013
	−24.99	0.362	188	1	0.021
$(CH_3)_4NI$	0.175	1.39	234	1	0.10
	−8.90	1.70	214	1	0.12
$(CH_3)_4NClO_4$	0.16	0.84	218	1.5	0.06
$(CH_3)_4NBF_4$	0.16	0.79	215	2	0.08
$(C_2H_5)_4NBr$	0.16	2.14	215	1	0.26
$(n\text{-}C_3H_7)_4NI$	0.175	3.85	197	0.5	0.43

TABLE 2*

QUANTITIES DERIVED FROM SHEDLOVSKY CALCULATION: PLANAR IONOPHORES

Solute	Temp. °C.	$10^3 K_{exp.}$ moles/liter	Λ_o mhos cm^2 $mole^{-1}$	(Variance)½ 95 per cent confidence limits (±) Λ_o	$10^3 K_{exp.}$
MePyrI	+0.175	1.38	231	2	0.15
	−8.90	1.75	211	2	0.20
$MePyrC10_4$	+0.175	0.95	214	1	0.05
C_7H_7Br	+0.16	1.26	220	0.5	0.05
$(C_6H_5)_3CC10_4$	+0.16	4.5	173	0.5	0.5
	−25.40	5.8	132	0.5	1.2
Me_4NPic	+0.16	1.95	175	1	0.20
MePyrPic	+0.175	2.9	168	1.5	0.7

*See TABLE 4 for meaning of abbreviations.

Discussion and Conclusions

Insertion of the data of TABLES 1 and 2 into Bjerrum's equation[7] yields "distances of closest approach," a_o, that are assembled in TABLES 3 and 4 along with estimates of the crystallographic or van der Waals radii (r_+ or r_-). For the tetraalkyl ammonium ions the radii were estimated for the most extended conformations from known bond distances and angles and the van der Waals radius of hydrogen. For an ion of planar symmetry the radius was identified with the maximum van der Waals radius about the center of gravity as determined from known bond distances and angles and atomic masses. This radius defines a spherical region occupied by the rotating ion.

From the Bjerrum a_o values a table of Bjerrum radii has been constructed with the aid of the assumption that the Bjerrum radius of an ion is not affected in this medium by the nature of its counter ion and that the Bjerrum and crystallographic radii of K^+ are identical, an assumption that appears to be justified by the data for the potassium halides.[6] The only serious discrepancy between radii of a single ion estimated from different solutes arises in the case of picrate ion. The radius deduced from methyl pyridinium picrate is 1.7Å larger than that calculated from tetramethyl ammonium picrate data. The former value is chosen largely because it differs but little from the van der Waals radius.

Since there are no data on absolute ionic mobilities in liquid sulfur dioxide, it is necessary to assign a value to one ion. Because Λ_o for KBr and KI differ very little from values calculated from crystallographic radii and viscosity (η) by means of Stokes law, EQUATION 2,

$$\Lambda^\circ = \frac{0.8147 \times 10^{-8}}{\eta} \left[\frac{1}{r^+} + \frac{1}{r^-}\right]$$

it is assumed that the Stokes (as well as the Bjerrum) radius of K^+ is identical with its crystallographic radius. TABLE 5 summarizes equivalent ionic conductances (λ_o) at 0° C. derived in this fashion. The only discrepancy between Λ_o values of a single ion derived from Λ_o values of different solutes involves Cl^-. The value obtained directly from KCl is 19 units less than that obtained indirectly from Λ_o data for triphenyl-

TABLE 3

BJERRUM DISTANCES FOR SPHERICAL IONS

Solute	Temp. °C.	Bjerrum a_o[a] Å	r_+ Å	r_- Å	$(r_+ + r_-)$ Å
KBr	− 5.25	3.31 ± 0.02	1.33[b]	1.95[b]	3.28
	−10.71	3.31 ± 0.03			3.28
	−15.56	3.33 ± 0.04			3.28
	−20.58	3.34 ± 0.02			3.28
	−24.99	3.41 ± 0.03			3.28
$(CH_3)_4NI$	0.175	5.54 ± 0.16	3.30[c]	2.17[c]	5.47
	− 8.90	5.68 ± 0.19			5.47
$(CH_3)_4NClO_4$	+ 0.16	4.63 ± 0.09	3.30[c]	3.0[d] (1.35)[f]	6.3 (4.65)
$(CH_3)_4NBF_4$	+ 0.16	4.56 ± 0.12	3.30[c]	2.8[d] (1.4)[g]	6.1 (4.7)
$(C_2H_5)_4NBr$	+ 0.16	6.8 ± 0.5	4.65[c]	1.95[b]	6.60
$(C_3H_7)_4NI$[e]	+ 0.175	10.0 ± 0.6	5.88[c]	2.17[b]	8.05

(a) Ranges correspond to (Variance)$^{1/2}$ values of TABLE 1.

(b) Pauling, L. 1940. The Nature of the Chemical Bond. :358. Cornell Univ. Press. Ithaca, N. Y.

(c) Calculated from bond lengths, bond angles, and van der Waals radii tabulated by L. Pauling, ibid., pp. 80, 84, 161, 164, 189.

(d) Estimated from the report of E. Wilke-Dorfurt. 1924. Z. Angew. Chem. **37**: 712 and from L. Pauling, ibid., p. 240.

(e) r_+ estimated for fully extended conformation of the *n*-propyl groups.

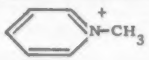
(f) Van der Waals radius of O.

(g) Crystallographic radius of F^-.

TABLE 4

BJERRUM DISTANCES FOR PLANAR IONS

Solute	Temp. °C.	Bjerrum a_o Å	r_+‡ Å	r_-‡ Å	$(r_+ + r_-)$ Å
MePyrI*	+ 0.175	5.5 ± 0.25	4.13	2.17	6.40
	− 8.90	5.7 ± 0.30			
MePyrC10_4*	+ 0.175	4.8 ± 0.11	4.13	3.0 (1.4)§	7.1 (5.5)
C_7H_7Br**	+ 0.16	5.3 ± 0.08	3.90	1.95	5.85
$(C_6H_5)_3CC10_4$	+ 0.16	11.1 ± 0.7	7.0	3.0 (1.4)§	10.0 (8.4)
	−25.40	10.5 ± 1.2			
Me_4NPic†	+ 0.16	6.5 ± 0.35	3.30	5.10	8.40
MePyrPic†	+ 0.175	8.3 ± 1.2	4.13	5.10	9.20

* MePyr+ = 

** C_7H_7+ =

† Pic^- = Picrate

‡ Radii of spherical ions chosen as in TABLE 3. See discussion for choice of radii for nonplanar ions.

§ Van der Waals radius of O.

methyl chloride[1,3] and perchlorate. The latter is preferred because it is in excellent agreement with the Stokes law value based on the crystallographic radius of $C1^-$, and because the low solubility of KC1 introduces experimental difficulties that are not encountered with other solutes.

From the limiting ionic conductivities of TABLE 5 and the appropriate form of EQUATION 2 there have been calculated a set of Stokes ionic radii (r_S). These are assembled in TABLES 6 and 7 along with corresponding Bjerrum and crystallographic or van der Waals radii.

These tables provide a convenient basis for discussion of the data. Agreement among the various radii of the anions is remarkable. Perchlorate and fluoroborate provide an interesting contrast between r_- and r_S on the one hand and å on the other. The å values are, however, in close agreement with the van der Waals radii of oxygen (1.4 Å) and fluorine (1.35 Å). Since, in the principal canonical forms of perchlorate and fluoroborate the oxygen and fluorine atoms bear single negative charges, the observed å

TABLE 5

LIMITING IONIC CONDUCTANCES AT 0° C.
BASED ON $\lambda^{\circ}_{K^+} = 152$

Cations		Anions	
Ion	λ°_{+}	Ion	λ°_{-}
K^+	(152)	Cl^-	110*
Me_4N^+	140	Br^-	96
$C_7H_7^+$	123	I^-	92
$MePyr^+$	138	ClO_4^-	76
Et_4N^+	118	BF_4^-	74
$n\text{-}Pr_4N^+$	104	Pic^-	33
$(C_6H_5)_3C^+$	96		

*Based on $(C_6H_5)_3CCl$.

values appear to constitute additional cases of adherence to Bjerrum's equation. The three radii of picrate ion are quite similar and may be taken as offering some degree of support for the procedure employed in assigning an appropriate van der Waals radius to a planar ion. The uncertainty with respect to a_o for this ion, however, diminishes the value of any conclusions.

The cations offer a somewhat more complex picture. Except for K^+, the

TABLE 6

COMPARISON OF IONIC RADII IN Å AT 0° C.: ANIONS

Ion	van der Waals radius r_-	Bjerrum radius a_o	Stokes radius r_S
Cl^-	1.81	1.63	1.84* (2.22)†
Br^-	1.95	1.95	2.10
I^-	2.17	2.20	2.20
BF_4^-	2.8	1.26	2.73
ClO_4^-	3.0	1.33	2.66
Pic^-‡	5.10	4.9	6.13

*Based on $(C_6H_5)_3CCl$.
†Based on KCl.
‡Based on MePyrPic.

Stokes radii are much smaller than the van der Waals radii although, with the exception of methyl pyridinium ion, the cations display the same sequences in r_+ and r_S. Curiously, it is the large ions that appear to deviate from Stokes law, although sphere in continuum theory should be most applicable where solute particles are larger than solvent molecules. The a_o and r_+ values agree fairly well with two exceptions, n-Pr_4N^+ and $(C_6H_5)_3C^+$. These a_o values were derived from the two largest reciprocal association constants measured, and consequently are most likely to suffer from the inadequacy[11] of Shedlovsky's method. In toto, these data emphasize the question that was raised by the earlier data:[6] Why does sphere in continuum theory work as well as it does for liquid sulfur dioxide solutions of 1:1 electrolytes?

TABLE 7

COMPARISON OF IONIC RADII IN Å AT 0° C.: CATIONS

Ion	van der Waals radius r_+	Bjerrum radius a_o	Stokes radius r_S
K^+	1.33	(1.33)	(1.33)
Me_4N^+	3.30	3.30	1.44
Et_4N^+	4.65	4.9	1.71
n-Pr_4N^+	5.9	7.8	1.94
$C_7H_7^+$	3.90	3.4	1.64
$MePyr^+$	4.13	3.4	1.47
$(C_6H_5)_3C^+$	7.0	9.8	2.10

These and the earlier data for KBr enable a determination to be made of the enthalpy of dissociation of this pair: $\Delta H = -4.77 \pm 0.34$ kcal/mole and appears to be constant from 0 to −25° C. The indicated uncertainty was calculated from a least square treatment of the lnK versus T^{-1} data using 95 per cent confidence limits as indicated previously. This value corresponds well to the value, −4.54 kcal, calculated[6] for 0° C. from Bjerrum's equation and $r_+ + r_-$. Bjerrum's ΔH, unlike the experimental value, increases with decreasing temperature, reaching −2.99 kcal/mole at −25° C.

In recent years some measure of success in accounting for ionic association in a variety of media has been obtained with the thermodynamic approach of Denison and Ramsey and elaborations of it.[11,14,15] It was noted earlier[6] that for data in liquid sulfur dioxide Denison and Ramsey's relationship does not yield the identity between $\hat{a}$ and $r_+ + r_-$ that Bjerrum's

treatment does. However, the extension suggested by Fuoss,[11] EQUATION 3, substantially recovers this identity. From a plot of log K_{exp}

$$K_{exp} = A\ e^{-\epsilon^2/aDkT}$$

versus $(DT)^{-1}$, a is found to be 3.1 Å. The corresponding value of A is 6.5, but a very long extrapolation is involved in its determination and it must be regarded as only an approximation. In any case the significance of A is obscure. It is also possible to apply Gilkerson's treatment[14] to the data for KBr. This theory yields values of distances of closest approach in other media that are much smaller than corresponding Bjerrum q values. This is observed in the present case also, the Gilkerson a value being 1.81 Å. The value found for E_S, a quantity which Gilkerson regards as the difference between solvent-ion and solvent-ion pair interaction is 3.18 kcal/mole.

Since the exponential part of EQUATION 3 is based on a sphere in continuum model, there is once more observed the unusual ability of such models to reproduce observed behavior in sulfur dioxide solution with a equal to $r_+ + r_-$, or very nearly so. The correlation of this phenomenon with the properties of liquid sulfur dioxide remains to be considered, as does the detailed interpretation of the behavior of the individual electrolytes.

Acknowledgment

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SECTION OF GEOLOGY AND MINERALOGY

MICROSEISMS: THEIR NATURE AND GEOLOGIC APPLICATION*

By William L. Donn

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Seismographs throughout the world show that the earth's crust quivers almost continuously. This quivering is registered on seismograms as a fairly common low-amplitude, irregular wave motion and is known generally as the "background noise." However, this background effect periodically erupts into very prominent, quite uniform sinusoidal waves whose amplitudes may mask the waves of small earthquakes. Because the period of the waves is usually from two to six seconds, compared to the longer periods of most of the earthquake waves, they are known as "microseisms."

Microseism "storms" may continue for several hours to several days and are usually correlated with the presence of wind or storm conditions over nearby large water bodies. The microseism effects of intense coastal cyclones can shake an entire continent. Thus, storms off the Alaskan coast generate microseisms that are recorded with relatively little attenuation at seismic stations over most of North America and as far distant as the eastern Atlantic coast of the United States. Cyclones off the coast of Europe have generated microseisms that have been noted over much of the Eurasian continent.

At present there is no generally accepted theoretical mechanism for the generation of microseisms in storm areas. The chief theories offered over the past sixty years can be stated as follows: (1) surf mechanism, or the shaking of shore areas by the breaking of high waves, resulting in the propagation of microseism waves; (2) progressive swell, or the pressure effect created on a sea floor by the passage of surface waves, the swell being transmitted as microseism energy; (3) atmospheric oscillations, or the impulsive effect of atmospheric turbulent motion on the water surface, transmitted to the bottom as sound noise and there converted into seismic energy; (4) interference of waves, or standing waves produced through interference of oppositely moving water waves of the same period, producing pressure changes on the bottom that propagate as microseisms; and (5) breaking waves, in connection with which I have recently proposed that noise from waves breaking at sea might be converted into microseism energy at the bottom.

The wave motion associated with microseisms seems greatly to resemble Rayleigh wave motion of earthquake surface waves, that is, the

*This paper, Lamont Geological Observatory Contribution No. 268, was to have been presented at the Section meeting scheduled for November 11, 1957, which was canceled.

motion appears to be that of a vertically polarized ellipse, with the ground particle moving in a retrograde sense in the ellipse. Some observers have described Love wave-type motion also, but this might be the result of interference between Rayleigh wave trains from different directions. If the orientation of the ground particle ellipse can be determined, this usually lines up well with the direction of the storm itself.

Studies of microseism propagation have afforded considerably more agreement than studies of their origin. The data so far accumulated suggest that microseisms travel with little attenuation in coherent layers of the earth's crust and that such layers act as wave guides for microseism energy. Microseism propagation seems to be similar to that for "channel" waves of earthquake seismology.

As mentioned earlier, propagation of microseisms across continents is very effective for storms over coastal waters. Also, propagation is very good along continental margins from storms present over marginal ocean areas. In contrast to these observations, no microseisms are recorded at either coastal or inland stations from very intense cyclones only a short distance eastward from the continental shelf of eastern North America.

In the same way, when hurricanes cross the West Indies Arc on a transverse path, microseisms at West Indies stations are recorded only when the hurricane is very close to land. However, propagation along the West Indies Arc appears to be more efficient.

These observations tend to suggest that the ocean basement does not propagate microseisms. However, the data from Bermuda, Guam, and other islands consisting of pure ocean-type crust show that microseisms generated over ocean areas do propagate well to ocean islands, but do not propagate to coastal stations of any nearby continent area.

Therefore, it seems that the continent-ocean margin or the island arc-ocean margin acts as a barrier to microseism propagation, but that propagation is fairly efficient within each unit.

A striking anomaly in the propagation along the eastern margin of the United States occurs in the vicinity of Cape Hatteras. Coastal stations north of this region record only weak microseisms from storms to the south, and the reverse. This effect is in accord with results of magnetic and explosion seismology studies that also suggest anomalies in this area.

In general, the data of microseism propagation seem to reveal gross structural trends within the earth's crust. As these data are available with little experimental expenditure, a systematic study may reveal regions worth investigating by more refined techniques.

SECTION OF BIOLOGY

LABILE BIOCOLLOIDS, CELL DIVISION, AND THE STRUCTURE OF THE MITOTIC APPARATUS*

By Paul R. Gross

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Oh, happy the man who can still hope
To rise out from this sea of error!
One needs precisely what one doesn't know,
And what one does know can't be used.
Goethe, *Faust*, Part I

Since the first statement of the universality of cell division by Remak and the pronunciation of the famous dictum, *omnis cellula e cellula* by Virchow[1] the problem of the mechanism of cell division has been a major concern of cytologists and physiologists. The spectacular microscopic events of karyokinesis could not fail to provide material for a vast descriptive literature, while the equally intriguing phenomena of cytokinesis excited unprecedented quantities of mechanistic speculation. Excellent coverage of the earlier literature is given in Wilson,[1] and Schrader's book[2] surveys the literature of 1952.

No field of biological inquiry has yielded such a plethora of hypothesis combined with such a frustrating deficiency of experimental proof. Perhaps the great accumulation of descriptive material on mitosis is responsible: such accumulations of fact are stifling to the maker of broad hypotheses while, simultaneously, the many explanations of events in specific cell types are doomed to failure by contradictions adduced through studies on other cells. A possible escape from this situation is to consider, as some have, that mitosis has not one mechanism, but a host of different ones, all evolved to accomplish the same end. I prefer an alternative approach, exemplified in the essays of Anderson[3,4] in which cell division is considered a necessity in the earliest evolution of cellular life, as a result of which some fundamental or primitive mechanism should be at least the basis for the division of contemporary cells.

This is not to say that all of the phenomena of cell division arise from a unitary cause: in fact, one of the earliest firm conclusions concerning mitotic mechanism is that of Boveri,[1] to wit, that the origin and assembly of the achromatic figure is physiologically separable from the mechanisms controlling chromosomal alterations. Many experiments have verified this conclusion; an elegant series in a classical material is that of E.

*This paper, illustrated with lantern slides, was presented at a meeting of the Section on November 11, 1957.

B. Harvey,[5] who studied division in fertilized and parthenogenetic merogones of sea urchin eggs.

All of the older general hypotheses are more or less unsatisfactory. Theories of contraction versus expansion of the spindle, electrostatic versus magnetic forces governing chromosome movements, tuned centriolar "resonators," diffusion streams, and surface tension effects: all have remained hypotheses because they have been untestable or have been shown to be untenable by experimentation. Not the least intractable of cytologic facts has been the series of descriptions of the curious "autonomy" of chromosomes, as seen, for example, by Metz[6] in *Sciara*. Even the relatively sophisticated description of the spindle as a tactoid appears to have been both premature and theoretically untenable.[2-4]

Recently, several new approaches to the problem of mitosis have been essayed. Encouraging results have been obtained, for example, from studies on the biosynthesis of DNA. The entire metabolic approach has been given impetus by the practical importance of discovering means for the control of cell division. However, we shall not be concerned here with studies on the metabolic events preceding or accompanying mitosis; these are fundamentally questions of energy flow and material supply for the life of the cell; we may hope that such studies will provide early practical means for interfering with mitosis, but it appears that the problem of structure formation in a structureless endoplasm cannot be solved by metabolic studies alone. It is precisely this problem of structure-assembly that is to concern us here.

Sol-Gel Transformations in the Endoplasm[7]

One of the surest possessions of cellular physiology is the observation that (in cells where it can be measured) the viscosity or, better, the rigidity of the cytoplasm is ordinarily low. The cell normally possesses a stiff cortex, but the resting endoplasm is fluid. To this must be added the observation that mitosis is preceded by and associated with a sharp increase in the cytoplasmic viscosity. Both Heilbrunn[8] and Danielli,[9] who disagree on basic mechanisms, regard the endoplasmic gelation as a necessary preliminary to the formation of the mitotic spindle. A large literature supporting the existence of these sol-gel transformations in the cytoplasm during mitosis is reviewed in Heilbrunn's book.[10] As will be shown, contemporary thought concerning the mechanism of assembly of the mitotic apparatus has included these observations on the physical state of the cytoplasm, regardless of differences among the commentators in regard to molecular events underlying changes in that state. It would thus appear profitable to examine first what is known about the mechanisms of generalized sol-gel equilibria in the endoplasm.

Many writers on cell physiology have accepted the idea, most elaborately developed by Frey-Wyssling,[11] that changes in the consistency and order

of the cytoplasm are effected by the reversible unfolding of globular proteins to produce networks of single or grouped linear polypeptide chains. Goldacre[12] has used such reversible unfolding as the basis for an ingenious theory of osmotic work and ameboid movement. Despite this, I am inclined toward the view that, if unfolding of protein molecules occurs at all in the bulk phases of the cell, it must be a phenomenon as unimportant for sol-gel equilibria as it is rare. The detailed refutation of theories invoking linkage of linear polypeptide chains in cytoplasmic sol-gel equilibria is beyond the scope of this paper; suffice it to say that not only are there no mechanisms known by which such reversible folding and unfolding could occur in cells, but no acceptable proof has been adduced showing that it ever does except, perhaps, in pathological specimens. The matter is discussed in Anderson,[3,4,13] whose conclusions are in agreement with statements made here, and whose alternative is the one I accept, namely, a reversible association of native, three-dimensional colloidal particles of the cytoplasm to form beaded chains and, ultimately, as necessity demands, true gels (exhibiting a yield value) and structure, by extensive cross-linking.

It is perhaps significant that while extended polypeptide chains exist in natural and artificial fibers, the fiber formation and *reversible* gelation of proteins that must retain some crucial biochemical activity (such as hormonal action) always involves association of the native, folded macromolecular "monomer" (cf. Waugh[14]).

The types of bond available for association among globular proteins and nucleoproteins are identical with those available for the junction points of extended polypeptides: Van der Waals-London forces, hydrogen bonds, protonic fluctuation forces (Kirkwood), ionic or "salt" linkages, and certain covalent bonds, such as dithio bridges. The strong dependence of processes involving reversible cytoplasmic sol-gel transformations on *p*H, ionic strength, and ionic composition suggest that the covalent bonds must ordinarily play a small, if any, part in the labile colloidal interactions, although they may serve to stabilize more permanent structures, such as the mitotic spindle. The ready reversibility of cytoplasmic gelation by increase in hydrostatic pressure suggests that a vast majority of the linkages between the aggregating units are electrovalent, since these bonds most characteristically show a large positive ΔV of formation.

Sol-Gel Transformations: Mechanisms

Heilbrunn and his students have provided much evidence that sol-gel transformations are initiated by a shift in the localization of ionic calcium within the cell.[10] They have demonstrated that the release of Ca^{++} from a bound state in the cell cortex is a general result of stimulation, and that such release produces, in turn, a gelation of the endoplasm. Danielli,[9] while agreeing that the sol-gel change may be initiated by calcium inter-

acting with endoplasmic proteins, makes a case against the primacy of Ca release in stimulation, arguing that, in order for bound Ca to be released from cortical protein, large changes in ion-binding capacity are required, and that the Heilbrunn school has not supplied evidence that such changes can be accomplished.

The point of this argument is well taken, that is, that one must specify mechanisms for *changes* in the cation-binding power of proteins near the cell surface; but it appears to me that mechanisms for accomplishing such changes are readily available. Thus, for example, many stimulants and parthenogens are acids, and can reduce the availability of anionic sites on cortical proteins; spermatozoa bring strongly basic proteins to the cell, as do some other parthenogenetic techniques, and these polycations could compete with Ca^{++} for ionized carboxylate sites on protein; electric fields across cell surfaces can produce such changes in proteins that their "preference" for one cation is altered to "preference" for another (as a result of alteration in the shape of the electron cloud about the anionic sites) and, indeed, recent preliminary data show that protein anions, under certain circumstances, can select the cationic free-amino groups of adjacent residues over free cations (Ling[15]).

Anderson has developed a very complete hypothesis on the mechanism of cytoplasmic sol-gel changes and structure assembly: in his hypothesis,[3,4,13] the role of inorganic divalent cations is minimized in favor of the organic polycations of the cell, such as basic proteins and smaller organic bases. This hypothesis will be discussed further in another section, but for the present it is clear that, whatever effects the polycations are supposed to have on endoplasmic macromolecules, the more readily diffusible inorganic polycations, especially Ca^{++}, should behave likewise. The point of general agreement is that sol-gel transformations in the cytoplasm must reflect the lability of certain colloidal complexes among which association-dissociation equilibria exist and that shifts in the position of equilibrium determine the colloidal properties of the cytoplasm. Such shifts depend, in turn, upon the availability of diffusible reactants possessing the power to alter charge or steric configuration on the surfaces of the sensitive macromolecules. Charges in configuration could be of the type of "reversible denaturation" described, in a recent paper, by Ungar and his associates.[16]

An important contribution to the problem was made by Hultin,[17,18] who showed that the addition of Ca^{++} to breis prepared from decalcified sea urchin eggs produced a series of reactions that mimic very closely the biochemical events concomitant with fertilization or stimulation of the normal cell: an increased uptake of oxygen, production of acid, lysis of cytoplasmic inclusions, unmasking of -SH groups,[16] and *an increase in viscosity*. All of these observations have been confirmed with material from the Woods Hole sea urchin, *Arbacia punctulata*, by us.[19-21] The importance of this result is that a colloidal change is initiated, among

materials of cytoplasmic origin, by the addition of an ion thought to be involved in the initiation of similar changes *in vivo*, and this change is accompanied by other biochemical events observed to accompany the sol-gel transformation *in vivo*.

It was our belief that the close correspondence of these phenomena with those occurring in the stimulated or injured cell (the "surface precipitation reaction" in the latter case) made it possible that elucidation of the mechanism of Hultin's viscosity increase might throw light on the mechanism of viscosity changes in the cell.

Our first investigations yielded the following information: recalcification (with physiological amounts of Ca^{++}) of homogenates of Ca-free eggs of *Arbacia* results in aggregation of nucleoprotein particles in the homogenate, and these particles, by virtue of their normal sedimentation properties, are probably much smaller than the mitochondria. The fraction that aggregates can thereafter be centrifuged down at low speeds; if left *in situ*, it produces a general clotting of concentrated homogenates. The fraction appeared to be electrophoretically homogeneous and, since it survives dialysis and further centrifugation, it was considered to be either a macromolecule in solution or a very small particle in suspension.

The aggregating material can be collected by suitable techniques and examined with the electron microscope; here it appears as a collection of spherical particles in a microfloc representing an extensively cross-linked gel. The particles have a mean diameter of 300-400 A.U., with a very broad distribution of size. Later work has shown that these particles are themselves clusters of two or three, with the unit diameter nearer to 140-160 A.U.

In further experiments,[21] more careful determinations of the quantities of PNA and protein in the sensitive material were made; it was shown that little or no Ca^{++} is bound to the aggregating material, suggesting that previous discovery of proteolytic activity in the homogenates[22,23] might be used to explain the aggregation process as an indirect, enzymatically mediated one, with the enzyme ion-activated. This explanation, however, has not yet been supported by adequate proof. It was shown that the fraction involved in the aggregation process accounted for a very significant part of the total PNA of the cell and, in consideration of its composition and physical behavior, was tentatively identified with the microsomes of the *Arbacia* egg.

In the meantime, morphologic studies on isolated liver "microsomes"[24,25] revealed that this fraction is heterogeneous, comprising vesicular structures apparently derived from the "endoplasmic reticulum," and a subfraction consisting of small RNA-rich particles like those observed in the ground cytoplasm by Palade.[26]

Our own studies on the cytoplasm of the *Arbacia* egg showed that, as in most embryonic or rapidly proliferating cells, there is no extensive endoplasmic reticulum, but the ground substance is very abundantly

supplied with 160 A.U. Palade particles. FIGURE 1 is an electron micrograph of a thin section of the cytoplasm of an unfertilized *Arbacia* egg.

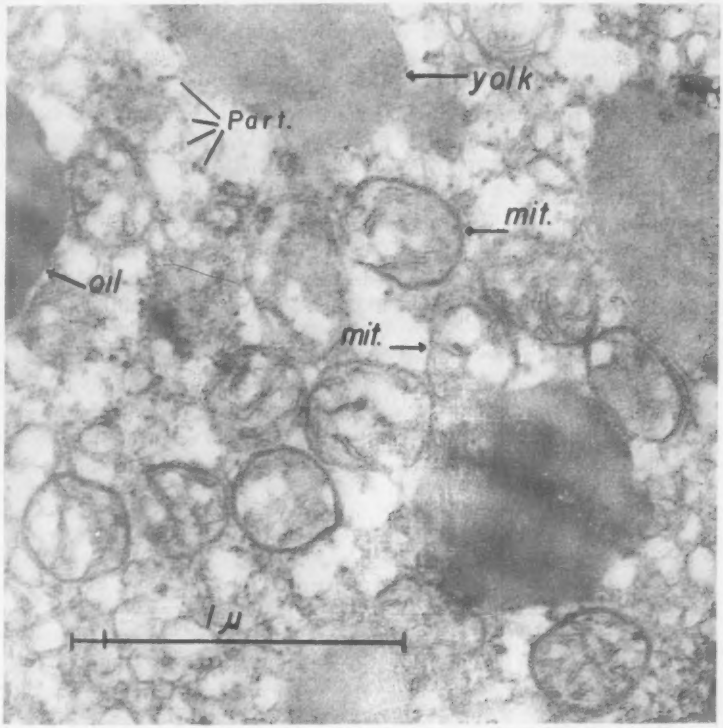


FIGURE 1. Electron micrograph of a thin section of the cytoplasm of an unfertilized egg of *Arbacia punctulata*. Note membranes surrounding yolk granules and mitochondria (mit.), internal membranes of mitochondria. Part. signifies dense, spherical particles found in profusion in the ground cytoplasm. The subdivision of the micron line represents 1000 A. Buffered, isotonic O_sO_4 fixation.

The Palade particles are shown, together with mitochondria, yolk particles, and oil droplets. Our most recent studies have identified these particles with those found in the microsome fraction obtained by differential centrifugation.[27]

While further localization and characterization of the "sol-gel" fraction, if it may be so named, was being attempted in sea urchin eggs, it became clear that if the mechanism being constructed from *in vitro* experiments was of any importance, it should be observable in cells other than sea urchin eggs. Certainly, the sol-gel transformation in living cells is very

widely distributed, and it would be unlikely for its mechanism to be totally different in sea urchin eggs and, for example, in mammalian tissue cells.

Examination of the literature indicated that Ca^{++} is a potent precipitant of nucleoprotein from water homogenates of rat liver; the most complete work is that of Schneider.[28] With the collaboration of William Pearl, of our laboratory, a program of studies was initiated, the purpose of which was to ascertain whether the reactions observed in sea urchin egg breis could be duplicated in liver homogenates. In order to make the experiments comparable, the livers were perfused and made blood-free with citrate-containing, Ca^{++}-free media, and all further reactions were carried out at physiological ionic strength. As is shown in detail in our forthcoming paper on this work,[29] the nucleoprotein aggregation reactions and concomitant biochemical phenomena are almost identical in sea urchin egg and rat liver. FIGURE 2, plotted from data published else-

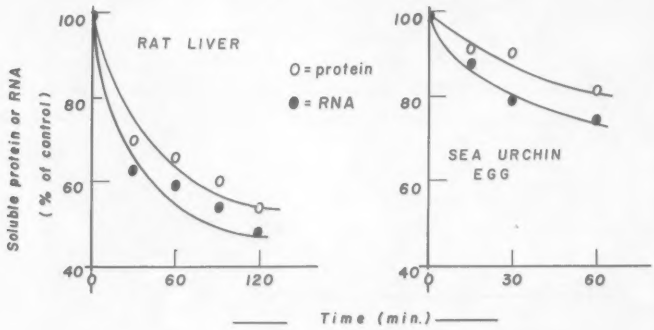


FIGURE 2. Kinetics of aggregation of nucleoprotein particles in breis of rat liver and sea urchin eggs. Note differences in abcissae. *Left-hand graph:* protein and PNA remaining nonsedimentable at 16,000 × g after addition of 0.025 M $CaCl_2$. *Right-hand graph:* protein and PNA remaining nonsedimentable at 25,000 × g for 5 min. after addition of 0.0098 M $CaCl_2$.

where, shows the loss in solubility of protein and PNA with time in rat liver and sea urchin egg breis after treatment with Ca^{++}. In both cases, "loss in solubility" means more precisely that material that does not sediment under identical conditions in Ca-free controls becomes sedimentable at about 16,000 × g.

We concluded that a possible mechanism for the cytoplasmic sol-gel transformation exists in these reactions. Accordingly, more complete characterization of the aggregating material was undertaken.

Microsomes, Soluble Phase, and Polycations

Identification of the Ca-sensitive fraction in both sea urchin eggs and rat liver requires a suitable fractionation of the brei. Since no such tech-

nique was available for the marine eggs, Sylvan Nass, of our laboratory, and I developed one, using isotonic KC1 as the homogenization medium. With our method, it is possible to separate clean fractions of pigment, yolk, mitochondria, microsomes, and soluble materials. In collaboration with Olov Lindberg of the Wenner-Gren Institute, Stockholm, Sweden, we were able to produce, for example, mitochondrial fractions with more than 90 per cent of the oxidative phosphorylation of the whole brei. All fractions have been examined with the electron microscope, and are clean by contemporary standards.[27] The microsomal fraction consists mainly of clumps of the Palade particles together with a few vesicular structures that are probably artifacts. Using these fractions, we undertook to determine which one gave the characteristic response with calcium.

After initial trails, attention was focused on the microsomes and the soluble phase, for both experimental and theoretical reasons. When the ionic strength is near what must be regarded as minimum physiological levels (0.5), only the microsomes aggregate in Ca^{++}, producing, as they do, a considerable *p*H drop. This "acid" must consist merely in a number of protons stripped from acidic sites on the microsomes, either by binding of Ca, or because these cationic sites ionize and then cross-link with sites of opposite charge. It is suggested here that this is the source of the well-known "acid of fertilization" (Runnström[30]). TABLE 1

TABLE 1

SOLUBLE PROTEIN AFTER TREATMENT OF FRACTIONS WITH 0.007 M $CaC1_2$. TOTAL IONIC STRENGTH: 0.53, MADE UP WITH KC1, IN CONTROLS (C) AND EXPERIMENTALS (X)

Fraction	*protein (mg.)*
Soluble phase C	7.45
Soluble phase X	7.68
Microsomes C	6.28
Microsomes X	4.76
Microsomes plus soluble phase, C	4.65
Microsomes plus soluble phase, X	3.72

shows the loss of protein from microsomal, soluble, and mixed microsome-soluble fractions of the egg at normal ionic strength. The microsomes are prepared by sedimentation in a preparative ultracentrifuge at 100,000 × g for 30 to 45 min. They pack poorly as a pink to red translucent pellet.

Reports from Mazia's laboratory[31,32] concerning a water-soluble, Ca^{++}-precipitable macromolecule, thought to participate in spindle assembly, prompted us to study these aggregation reactions at lower ionic strength. As is shown in TABLE 2, both the soluble phase and the micro-

TABLE 2

SOLUBLE PROTEIN AND PENTOSE AFTER TREATMENT OF FRACTIONS WITH 0.01 M. $CaCl_2$. FRACTIONS DIALYZED AGAINST DISTILLED WATER. FINAL *pH* IN REACTION MIXTURES: 6.1

Fraction	*Protein (mg.)*	*Pentose (μg)*
Soluble phase C	11.2	142
Soluble phase X	5.95	41.6
Microsomes C	6.43	123
Microsomes X	1.66	22.7

somes produce instantaneous precipitates with Ca^{++} at very low ionic strength. Clearly, the reaction is strongly salt dependent, indicating the operation of a kinetic salt effect in the reaction of oppositely charged ionic species. In FIGURE 3, the rate of precipitation of nucleoprotein from the *soluble phase alone* is shown as a function of the square root of the ionic strength, *s*. The vertical double line in the graph shows the position of minimal normal ionic strength in the cell. It is thus very unlikely that the rapid precipitation of soluble-phase nucleoprotein can occur *in vivo;* indeed, at $s = 0.5$, this material does not precipitate with calcium at all. The microsomal precipitation, on the other hand, takes place, albeit slowly, at high ionic strengths, and this very slowness, with formation of aggregates and gels rather than precipitates *in vitro,*

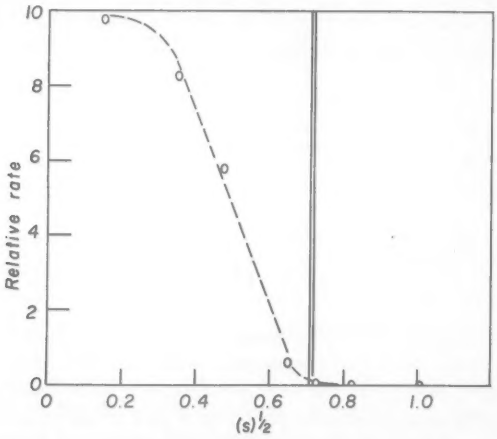


FIGURE 3. Relative rates of precipitation of nucleoprotein from the soluble phase of a sea urchin egg brei as a function of square root of total ionic strength. The vertical double line shows the assumed position of minimum intracellular ionic strength. All samples treated with 0.008 M $CaCl_2$. Final ionic strength achieved by addition of KCl.

suggests that it is the microsomal, and not the soluble phase reaction with Ca^{++}, that may be of physiological importance.

Exactly the same effect is observed in material obtained from rat liver breis. At normal ionic strengths a slow, approximately first-order precipitation of microsomal material takes place: at very low ionic strengths, a massive and instantaneous precipitation of nucleoprotein occurs, and much of the precipitated material comes from the soluble phase. In liver, the kinetics of the process follow approximately the Brønsted equation for a primary kinetic salt effect:

$$\log_{10} k = B + 1.018\, z_a z_b \sqrt{s}$$

where the z's are the charge numbers of the ionic reactants and s is the ionic strength.

Results of preliminary experiments performed in our laboratory by Bernard Moss show that in homogenates of liver centrifuged to sediment all uricase activity (and hence mitochondria and "lysosomes") the addition of calcium in small quantities to the supernatants precipitates particles bearing the glucose-6-phosphatase activity. This enzyme is known to be strongly localized in the microsomes.[33] Sjöstrand[34] has published electron micrographs of isolated microsomal material from liver; the fraction contains vesicular derivatives of cytoplasmic membrane systems[24,25] and "small" microsomes that appear to be clumps of the Palade particles.

We conclude that *in vivo* the most likely colloidal reactant with Ca^{++}, or indeed with polycations in general, will be the small microsomal particle. It is perhaps of interest to observe that the pentose-protein ratio of the soluble-phase, Ca-precipitable macromolecule is very close to that of the microsomal fraction as a whole in sea urchin eggs, suggesting it as a possible soluble precursor of microsomes. Such a suggestion can be tested with tracer techniques.

The Mitotic Apparatus

The most important recent contributions to our knowledge of the chemistry of mitosis have been the skillful development of techniques for the isolation of mitotic spindles from dividing eggs by Mazia and Dan,[31,32,35] and the detailed investigation of this material and its possible cytoplasmic sources by Mazia and his students. The general results of this imaginative work have been as follows: the separate existence of the mitotic figure, and more specifically, the achromatic figure, as a highly gelled, ordered structure have been made more certain. The polarization microscopy of Inoue[36] and the micurgical experiments (for example, Carlson[37]) had already strengthened the case almost beyond doubt. Mazia's general description of the events of mitosis are: (1) a generalized cytoplasmic gelation; and (2) a local ordering of the diffuse endoplasmic gel, probably

by a secondary bonding agent secreted by the centrosomes and kinetochores, to produce the amphiaster.

Observations on sectioned and osmicated isolated spindles showed a diffuse, fibrous structure, with the fibers poorly, if at all, resolved. This suggests that the spindle is produced by fiber-formation of soluble macromolecules in the ground cytoplasm. Mazia proposed that the gelation reaction is produced by disulfide interchange as described by Huggins[38] for serum albumin; later, evidence was adduced to show that the mitotic cycling of glutathione, first described by Rapkine,[39] is mirror-imaged by a cycling of protein-bound -SH.

Chemical investigation of the material of spindles dissolved, after isolation, in alkaline thioglycollate showed that most of the spindle material could be accounted for by a single, simple protein, containing a small amount of one nucleotide, possibly adenylic acid.

In experiments with Kane, Mazia showed that if eggs are killed with subzero 30 per cent ethanol and extracted with water, a soluble protein having the properties of the "spindle precursor" undergoes cycles of aggregation coinciding with the formation and dissolution of the mitotic spindle.

This very thorough analysis is perhaps not done justice by so hasty a summary, but its general import is as follows: the change in endoplasmic rigidity that heralds mitosis is produced by the aggregation of a soluble protein of cytoplasmic origin and this relatively simple molecule forms fibers and later structure by disulfide-interchange. The precursor of the spindle must, at least in the sea urchin egg, come entirely from the cytoplasm, since the nucleus is too small to contain it; 11 to 12 per cent of the cytoplasmic protein is invested in the spindle.[40]

Anderson, in his interesting discussion of the theoretical aspects of cell division,[3,4] concludes from general considerations that the spindle precursor should be a soluble protein. He suggests, however, that the disulfide interchange hypothesis is weak, since the properties of an extensively S-S cross-linked gel do not coincide with the properties of the mitotic apparatus: its lability to high hydrostatic pressure, for example, and the sensitivity of cytoplasmic gels to changes in ionic strength and composition. Anderson concludes that the spindle, and all other cytoplasmic structure, represents a dynamic equilibrium among labile colloidal complexes, whose aggregation into beaded chains, fibers, and sheets is controlled mainly by ionic bonds and initiated by alterations in polyelectrolyte balance. For Anderson, the initiation of cell division resides in an increase, probably of metabolic origin, in basic proteins.

I find objections to both these points of view, not neglecting, however, the great value of the experimental results obtained by Mazia. So far as Mazia's hypothesis is concerned, several difficulties arise: (1) the evidence for disulfide interchange and for S-S cross-linking of the spindle *in vivo* remains circumstantial and the evidence presented is open to

alternative interpretation; (2) a critical test of the soluble-phase precursor hypothesis has been made in Mazia's own laboratory. In a well-designed experiment determinations of total soluble and total insoluble protein were made on dividing and nondividing cells. If the hypothesis is correct, and about 12 per cent of the total soluble protein is invested in the spindle, metaphase cells should show a relative loss in soluble and a gain in insoluble protein. The results show no such shift: indeed, unfertilized and metaphasic eggs showed identical amounts of soluble protein (average 47 per cent). This result, if it is to be incorporated into the Mazia hypothesis, requires that an amount of insoluble protein exactly corresponding to the quantity of soluble protein brought into the spindle be released in soluble form from structure some time between the initiation of division and the appearance of the spindle. Furthermore, this protein must replace the spindle precursor in the soluble phase – it cannot itself enter the spindle. These ideas are somewhat in accord with suggestions of Anderson based upon certain observations of dissolving mitochondria during cell division, but they appear to me to be questionable.

Mazia has found that a protein extracted with water from powders of alcoholized eggs precipitates with Ca^{++} and has other properties of the spindle precursor; he suggests that this protein may in fact be of such nature and, by implication, that it is the "sol-gel" protein of the endoplasm. It appears quite likely to us that this protein is identical with the soluble-phase nucleoprotein that we have precipitated with Ca^{++} at low ionic strength, but it appears unlikely that it could be involved in a similar precipitation or fiber-forming process *in vivo*, for reasons mentioned in an earlier section.

With respect to Anderson's hypothesis, we note that all processes that he proposes to be initiated by polycations – probably basic proteins and/or organic bases – can be initiated equally well by Ca^{++}. Therefore, it is likely that, even if Anderson were correct in his supposition that it is organic rather than inorganic cations that initiate and control cytoplasmic sol-gel transformations (a conviction that we cannot share until more evidence is brought forward), the labile colloids of the cytoplasm that would aggregate and disaggregate first would be particles similar to the microsomal particles studied in the work described earlier in this discussion.

A final criticism of the Mazia hypothesis relates to the simplicity of the isolated spindle as seen with the electron microscope. Assuredly, the structures obtained after isolation are simple; very thorough chemical evidence on this score is available, making the morphologic observation almost unnecessary. However, we note that isolation of the spindle involves two treatments that are likely to disaggregate nucleoprotein particles: alcoholization and solubilization with detergents or with ATP (alcohol dissolves lipides). These treatments could well dissolve away

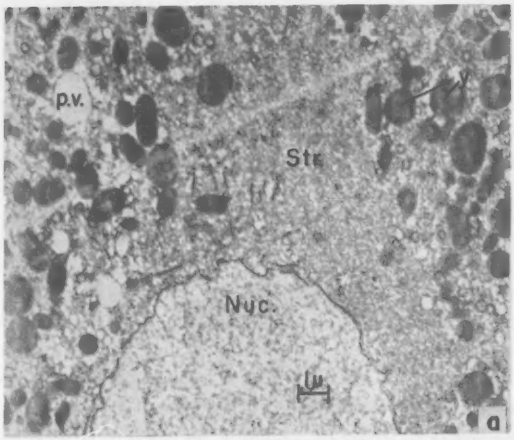
important constituents of the spindle, leaving a residue that has, perhaps, physiological significance but does not tell the entire story.

There exists, in fact, a large cytochemical literature that shows that the spindle is complex, containing, in addition to protein, RNA and polysaccharides,[41-43] The RNA of the spindle can be hydrolyzed with RNA'ase, which removes the basophilia observed in stained sections.[44] These properties would not be possessed by the simple nucleotide-protein of Mazia's structure. I believe it likely that the material of Mazia's spindle is an important component of the normal structure, possibly the one accounting for the fibers seen in O_sO_4-fixed cells in division by many observers, and discussed most recently by Porter,[45] but there must be other components of the spindle *in vivo*, perhaps important from the point of view of its assembly by the cell.

Electron Microscopy of the Spindle of Sea Urchin Eggs

FIGURE 4 shows two stages in the formation of the mitotic spindle in *Arbacia punctulata*. The photographs, and the others shown, were made at the Marine Biological Laboratory in Woods Hole in collaboration with Delbert E. Philpott. These studies are described in detail in a paper to be published shortly. FIGURE 4a shows a late "streak" stage that precedes prophase; 4b shows the fully developed metaphase spindle at low power. These cells were fixed in buffered isotonic 1 per cent O_sO_4. This fixative produces a high degree of dispersion of spindle structures, a result opposite to that produced by alcohol as a fixative (see below). The absence of *metaphase* chromosomes is not accidental; they are never seen in osmicated cells until early anaphase, when they begin to vesiculate. This condition is in agreement with a finding of Mitchison and Swann,[46] who state that at metaphase the chromosomes disappear in the living egg and have the same refractive index as the surrounding material of the amphiaster. For the present discussion, the important feature of these photographs is the exclusion of larger cytoplasmic particulates from the spindle and from the very early spindle seen in FIGURE 4a, without any obvious formation of fibers. Now, while we have evidence that fibers *are* in fact present in the fully formed spindle, these photographs suggest very strongly that there has occurred a generalized gelation of the endoplasm in the region of the spindle; furthermore that, except for the absence of the large inclusions (pigment, yolk, mitochondria), *the structure of the spindle region is identical with that of the ground cytoplasm elsewhere*. At the low magnification of FIGURES 4a and b, they are not immediately obvious, but use of a hand lens will show that this ground cytoplasm contains Palade particles, or microsomes, in great profusion.

The absence of any differentiation other than the presence of fibers between the peripheral ground substance and that in the spindle has been



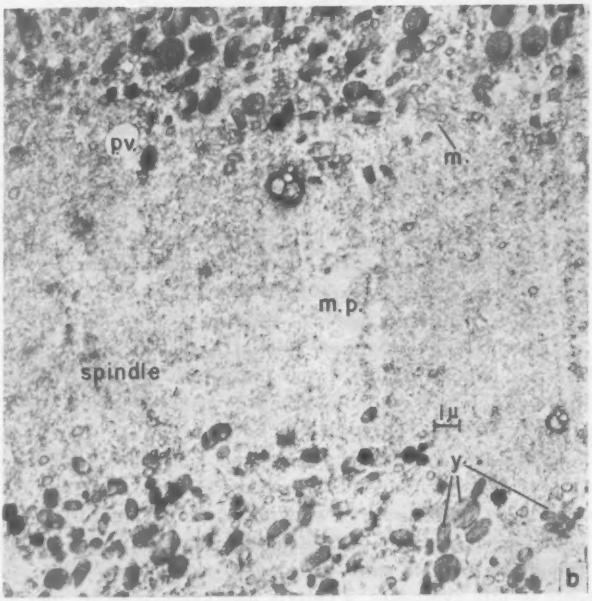


FIGURE 4(a). A late "streak" stage preceding prophase. Legend: p. v., pigment vacuole; Str., spindle "streak"; y., yolk particles; Nuc., nucleus. Fibrous structures in the spindle region are nuclear membrane fragments found in the perinuclear area also before fertilization. (b) Electron micrograph of metaphase spindle in first cleavage of *Arbacia*. Buffered, isotonic O_sO_4 fixation. Legend: m.p., metaphase plate; p.v., pigment vacuole; m., mitochondria; y., yolk particles. In this photograph (and in 4a), spindle regions have the same fine structure as does the ground cytoplasm outside the spindle.

observed by other investigators of mitotic figures; the sea urchin egg is uniquely favorable in this regard because the exclusion of large particles demarcates the amphiaster very sharply. It is interesting to recall that Mitchison and Swann[46] report that the refractive index of the spindle is probably identical with that of the ground cytoplasm if the contribution to the latter of the large particulates is subtracted.

As a check on the possibility that an extensive extraction of material has been effected in the isolated spindles observed with the electron microscope by Mazia,[31] we have fixed dividing cells in the solution used by Mazia and Dan[35] to kill their cells before spindle isolation. This is a 30 per cent alcohol solution chilled to --10° C. The cells were then osmicated in the cold, sectioned, and observed. Thus we could observe spindles treated just as were Mazia's,[31] except that the solubilization procedures were avoided. FIGURE 5 is a low power view of a very early anaphase spindle. The presence in this spindle of clumps of dense particles is clear; and it is therefore probable that these particles were solubilized in preparations of isolated spindles. The spindle shown also contains fibers, but these are seen to better advantage in a thinner section at higher power, as shown in FIGURE 6. This photograph shows clearly that, at least in spindles treated with osmium, definite tubular fibers (fi.) occur. This finding is in harmony with that of Porter[45] for mammalian cells, and with those of many other observers.[47,48] We are thus forced to conclude that the spindle is a complex structure containing *at least* two important components: the RNA-rich particles of the ground cytoplasm and a fibrous component, probably involved in chromosome movements, originating perhaps from a soluble phase precursor, as considered by Mazia and Anderson. In fact, this fibrous component may be the resistant material that remains in Mazia's isolated spindles and, if such is the case, then the extensive information obtained by the California group is immediately applicable to the origin and function of this one of the two major components, possibly the more important one for an understanding of chromosome movements.

It is necessary to conclude, however, that components of the ground cytoplasm other than simple soluble proteins play a role in the assembly of the spindle; as an example of such a possibility, FIGURE 7 is presented. This combines the possibilities arising from the studies on sol-gel transformations *in vitro* and on the morphology of the spindle. With respect to this synthesis, a certain ambivalence on my part must be admitted. The events underlying sol-gel transformations and spindle assembly, as presented in FIGURE 7, do explain both the submicroscopic morphology of spindles as we have seen them and the data on nucleoprotein aggregation *in vitro*. Such a fit provokes suitably affirmative feelings but, at the same time, the hypothesis does suffer from a certain esthetic insufficiency: it seems inelegant that microsomes must first

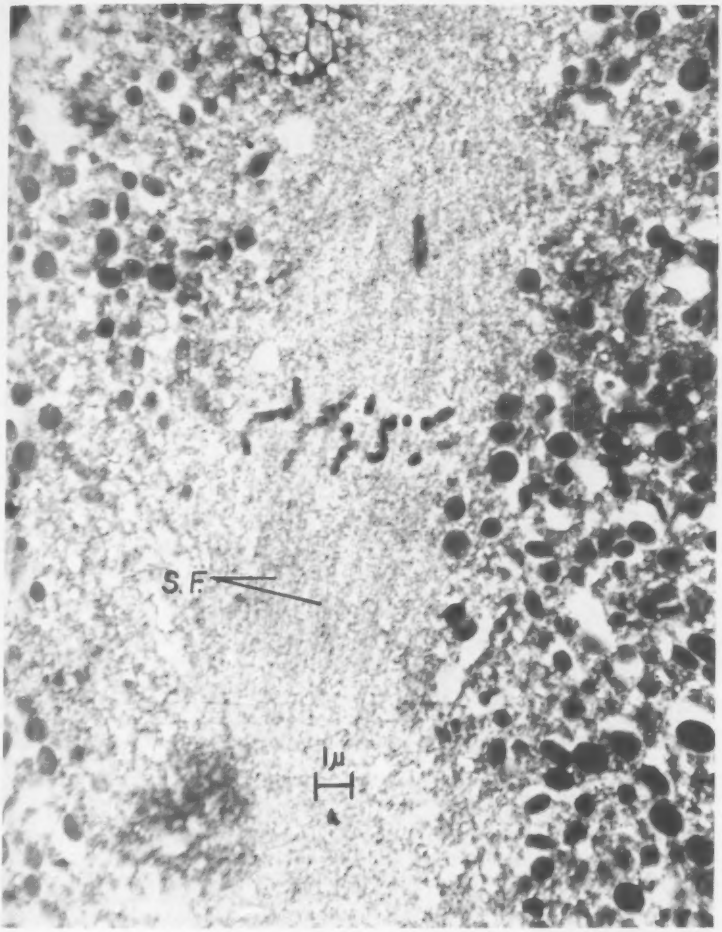


FIGURE 5. Low-power electron micrograph of an early anaphase figure in first cleavage of an *Arbacia* egg. S.F., spindle fibers. Fixation: 30 per cent ethanol at -10° C., with postosmication.

fibrilize to produce the spindle gel and that only then do other fibers, perhaps the chromosomal variety, form, possibly from Mazia's precursor protein, to complete the structure.

However nature *is* often complex, and she frustrates the hypotheses of biologists all too often, for this and other reasons. Thus while the hypotheses, including this one, are soon forgotten, the data often remain

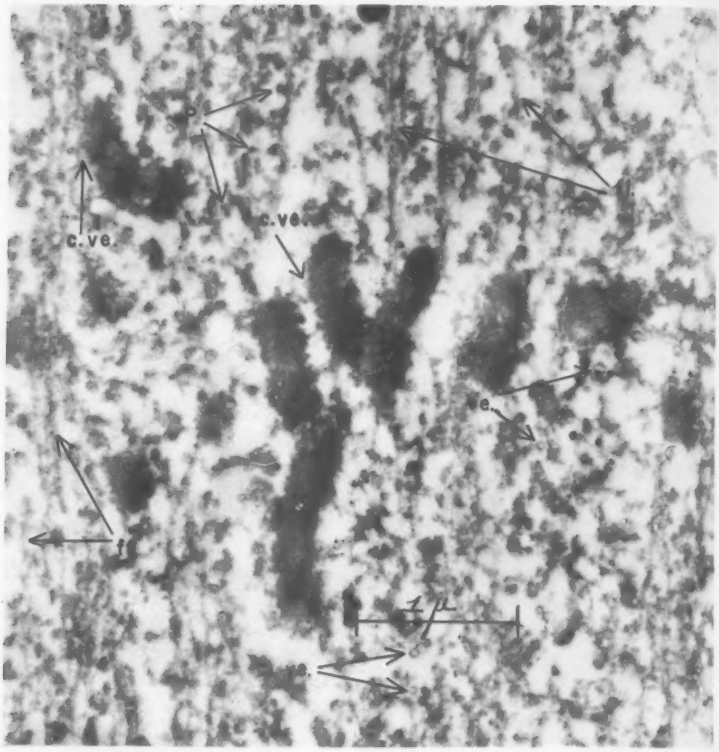


FIGURE 6. Metaphase plate region, first cleavage in an *Arbacia* egg. Electron micrograph of a cell fixed with 30 per cent ethanol at -10° C., followed by subzero ethanol containing 1 per cent O_sO_4. Legend: c.ve. chromosomal vesicles; ve., vesicular structures in the spindle, found also in the ground cytoplasm of unfertilized eggs and in the peripheral cytoplasm of the cell shown; fi., tubular fibrous structures, possibly chromosomal fibers, studded with clumps of Palade particles (P), which are also present in the nonfibrous regions of the spindle.

useful long after the concepts fabricated from them are outmoded. Perhaps such utility may be found in some of the newer data here presented.

Acknowledgment

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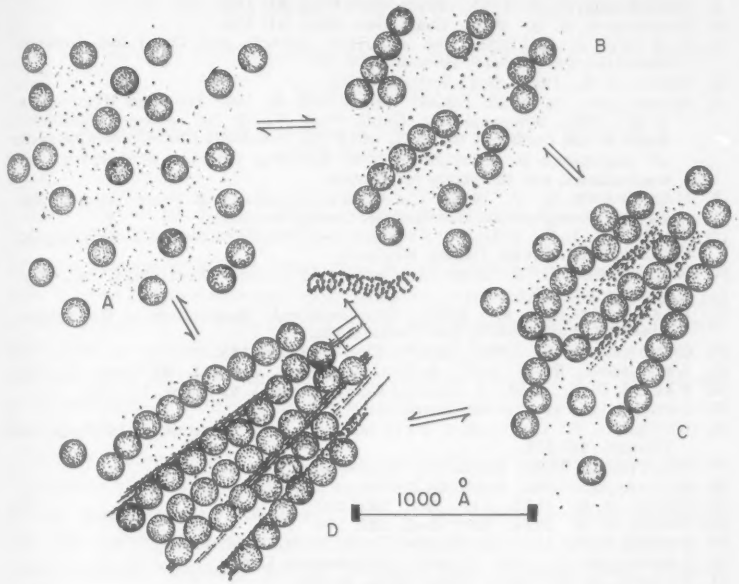

FIGURE 7. Hypothetical events in an endoplasmic sol-gel transformation and in assembly of spindle structure. In *A* particles such as are found in the "small microsome" fraction of the cytoplasm and described in the ground substance of many cells by Palade are present free in the endoplasm. The smaller particles represent soluble proteins and nucleoproteins. In *B* the larger nucleoprotein particles, that is, the small microsomes, aggregate in a linear fashion to produce short-chain "polymers." The endoplasmic viscosity would increase sharply. In *C* the necklace adlineation continues; cross-linking, perhaps by covalent bonds, is beginning. This process would be confined to the spindle region, and the condition shown would represent a true gel. Certain of the soluble macromolecules are shown undergoing their own aggregation to produce fibers running between and among the chains of Microsomes. In *D*, the spindle is presumed complete, the fibrous elements are depicted as "tubular" in consequence of a helical beaded-chain structure. Such fibers could contract by alteration in the pitch of the helix, the latter being initiated by changes in the ionic atmosphere. The linkages are unspecified and could be hydrogen bonds, salt linkages, or covalent bonds, but the weaker forces of the ionic and hydrogen bonds are preferred in view of the demonstrable lability of endoplasmic sols and gels and of the spindle itself.

versity, New York, N. Y. Special thanks are due to Delbert E. Philpott for his sympathetic collaboration in the electron microscopy, and to my graduate students, who have pursued their own interests with vigor in spite of my lucubrations.

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SECTION OF MATHEMATICS AND ENGINEERING

AN APPROACH TO GENERALIZED DESIGN OF STILLING BASINS*

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The purpose of stilling basins is to protect the flow channel downstream of spillways, sluice gates, and drop structures against harmful erosion. A simple stilling basin consists of a concrete apron and an end sill. A more elaborate design also includes chute and baffle blocks. Most of the energy of the high velocity flow that enters the stilling basin is dissipated in a hydraulic jump, generated and stabilized on the apron by the concerted action of the tailwater, baffles, and end sill. The design is satisfactory if a flow of small velocity and erosive action passes into the downstream channel.

The design methods in current use are based on model studies and on experience with existing structures. They vary not only with the type of stilling basin but also with the individual approach of various investigators who established these methods. It is, therefore, quite difficult to compare these empirical design methods, and to reveal their relative merits.

This paper is an attempt to establish certain common standards for stilling basins, and to provide an approach to a generalized design method. By studying a stilling basin of the simplest type, consisting of a horizontal apron and continuous, vertical end sill, the functional relationship between flow characteristics and the basin's geometry has been established. Using the characteristics of this basin as a standard, an analysis and comparison of current design methods prove to be possible. This leads to an application of existing methods with more understanding and flexibility.

The principles established in this article can be used for the design of stilling basins in conjunction with existing methods, or as a guide for model studies. They can also be used for design purposes independently of existing methods. However, it is felt that additional research is still needed to establish definitely a general, uniform design method for the basic types of stilling basins.

Formulation of the Problem

The stilling basin chosen for the present study is shown in FIGURE 1. The supercritical flow that enters the basin through the entrance section *a* is produced by the adjacent spillway, weir, drop structure, or

*This paper, illustrated with lantern slides, was presented at a meeting of the Section on November 15, 1957.

sluice gate. The resultant hydraulic jump is considered to be in the basic position if it begins at the entrance section *a*. A submerged entrance is possible if the tailwater is sufficiently high. The other possibility – the hydraulic jump beginning farther downstream from the entrance – should not occur, as it would increase the length of the basin unnecessarily.

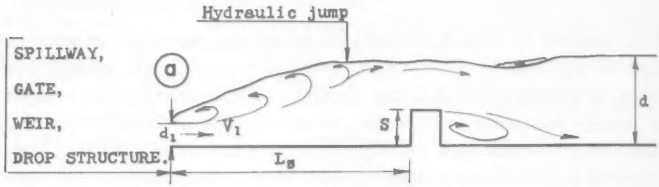

FIGURE 1. Elements of a stilling basin.

The hydraulic jump in the basin depends on the entrance flow of depth d_1* and the average velocity V_1. It would occur in the basic position for various relationships between the basin's length L_s, the height of the sill S, and the tailwater depth d. As is generally known, the dynamic similarity for stilling basins can be expressed by the Froude number and appropriate geometrical ratios. Consequently, the dimensionless ratios

$$F = \frac{V_1}{\sqrt{gd_1}}, \quad \frac{L_s}{d_1}, \quad \frac{S}{d_1}, \text{ and } \frac{d}{d_1} \tag{1}$$

can be used in the present study. The determination of their relationships for a satisfactory hydraulic performance of the stilling basin is based on the experimental data, discussed below.

Experimental Procedure and Data

The experimental data were gathered in connection with a study of straight drop stilling basins. That part of this study that deals with the spillway characteristics of a straight drop has been published earlier,[1-3] together with a detailed description of the two-dimensional experimental model.

A vertical drop in a rectangular plate glass flume was used to produce the supercritical entrance flow of a stilling basin. The basin consisted of a smooth, horizontal, concrete apron, and of a steel sill three fourths of an inch wide of variable position (L_s) and height (S). The tests were performed with the hydraulic jump in the basic position (FIGURE 2). The

*The letter symbols are defined where they first appear and in the *Glossary*.

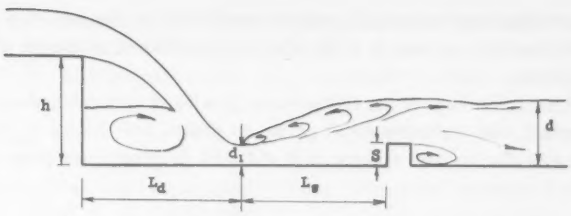

FIGURE 2. Straight drop stilling basin.

possible variation of L_s, S, and d for this condition was investigated over the range of $F = 3$ to $F = 7$. On the basis of some incomplete data, and by theoretical computations, the resulting graphs are completed to $F = 10$.

The bottom of the flume downstream of the sill was also of smooth concrete, and no erosion studies could be made. However, comparison of the observations of the flow pattern downstream of the sill with the erosion studies of other investigators helped to establish conclusions regarding the erosive action of the flow.

The experimental data have been organized into dimensionless graphs. As an example, the plot for $F = 4$ is given in FIGURE 3. The experimental points, corresponding to the same S/d_1 values, are indicated by partially drawn dashed lines. The full lines of S/d_1 = const. are drawn by interpolation. The values that correspond to these full lines are used in all subsequent graphs drawn in connection with the analysis of the data. Consequently, few experimental points are plotted in other graphs, and repeated interpolations and adjustments of experimental values have been avoided.

The data in FIGURE 3 represent many physically different flow types.

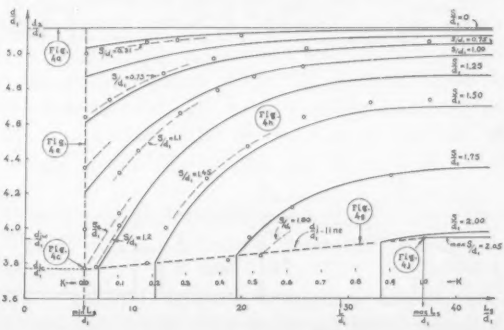

FIGURE 3. Experimental data, $F = 4$.

These are shown in FIGURE 4, and referred to in FIGURE 3. For a better interpretation of FIGURE 3, a detailed description of characteristic flow types follows.

In FIGURE 4a there is no sill present ($S = 0$). A natural hydraulic jump of a length L and tailwater depth d_2 takes place.

FIGURES 4b, c, and d show that the sill is placed as close as pos-

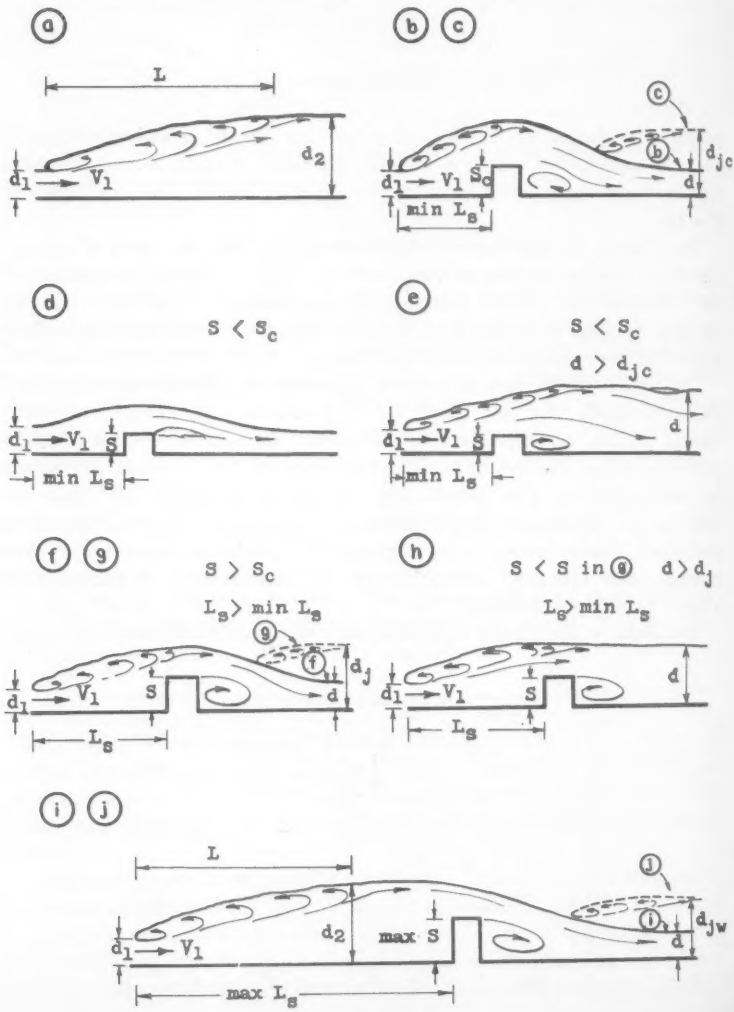

FIGURE 4. Flow types.

sible to the entrance of the basin (min L_s). A short, steep, forced hydraulic jump of rather limited energy dissipation is produced by the action of the sill (FIGURE 4b). The supercritical flow downstream of the sill does not affect the jump. Any reduction in this critical height of the sill (S_c) would result in a shooting flow over the sill, without any turbulent surface roller (FIGURE 4d). For a basin's length that is smaller than min L_s there are two possibilities: a submerged entrance for S_c, or no surface roller at all for a somewhat lower sill. Consequently, min L_s is the shortest possible basin length for the hydraulic jump in the basic position.

The tailwater can be raised to the depth d_{jc} before the jump is affected (FIGURE 4c). The resulting flow downstream of the sill shows a surface roller of moderate size. This roller is a remnant of the hydraulic jump that takes place as d increases to reach d_{jc}. There is an intensive bottom roller just downstream of the sill.

FIGURE 4e shows that by decreasing the height of the sill and keeping it at min L_s, an increased tailwater depth d is needed to keep the jump in the basic position. The jump reaches over the sill, the surface roller downstream of the sill decreases, as does the bottom roller, due to the reduced height of the sill. As S is decreased further, the hydraulic jump reaches further over the sill, and for $S = 0$ a tailwater depth of the natural hydraulic jump d_2 is needed.

In FIGURES 4f and g the basin's length is greater than min L_s and, to keep the jump in the basic position, a sill of $S > S_c$ is needed. The supercritical tailwater does not affect the jump (FIGURE 4f). If the tailwater depth is raised to d_j it would just start to affect the jump. A small surface roller is then seen downstream of the sill (FIGURE 4g). The bottom roller is intensive and large in size due to increased S. The hydraulic jump in the basin is very turbulent, and it is an effective energy dissipator.

FIGURE 4h shows that by decreasing S from what it is in FIGURE 4g a greater tailwater depth than d_j is needed to keep the jump in the basic position. The hydraulic jump in the basin remains turbulent and efficient. The flow conditions downstream of the sill improve. There is no surface roller, and the flow is relatively calm, exerting no strong action on the bottom. There is a bottom roller downstream of the sill, depending in size on the height of sill. The central part of FIGURE 3 corresponds to this flow type.

As the sill continues to decrease, the hydraulic jump reaches farther over the sill until for $S = 0$ a natural hydraulic jump with the tailwater depth d_2 takes place.

In FIGURES 4i and j, if the basin's length is max L_s, the sill of the height max S acts like a weir. The depth of flow upstream of the sill is then equal to d_2. If the sill is moved further downstream, the jump will still stay in the basic position, because the depth d_2 upstream of the

sill is kept constant by the weir-like sill. The tailwater can be supercritical (FIGURE 4i). It can be raised to the depth d_{jw} without any effect on the jump (FIGURE 4j). In the latter case the surface roller downstream of the sill looks more like another hydraulic jump. The bottom roller is high, but not too intensive. The flow action on the bottom downstream of the sill is very strong, as below a weir.

If the height of sill decreases at the position of max L_s, a depth above d_{jw} is needed to keep the jump in the basic position. The flow downstream of the sill is then calmer, without a strong bottom roller and without a strong action on the bottom. The decrease of S does not have any effect on the length of the hydraulic jump in the basin, because it has already reached its full development. The length of the natural hydraulic jump L is considerably shorter than max L_s, because a certain distance upstream of the sill is needed to develop the depth d_2.

As mentioned before, all the flow types in FIGURE 4 that have a hydraulic jump upstream of the sill correspond to a certain point or region in FIGURE 3. It can be seen that for $S > S_c$ a relatively high, subcritical tailwater is needed to keep the jump in the basic position. In the cases of S between S_c and max S, and L_s between min L_s and max L_s, a hydraulic jump can be produced in the basin independently of the tailwater. Accordingly, vertical lines of S/d_1 = const. in FIGURE 3 indicate the increase of the tailwater to d_j/d_1 without any other changes. The dashed d_j/d_1 line between d_{jc}/d_1 and d_{jw}/d_1 shows the lowest tailwater level that has any effect on the jump.

Only certain regions in FIGURE 3 and the corresponding flow types in FIGURE 4 are acceptable for stilling basins. Such regions will be defined after the data have been analyzed.

The Analysis of Data

Some of the experimentally determined ratios can be computed by known methods. The ratio d_2/d_1 can be expressed as a function of Froude number, derived by application of the momentum principle. The theoretical values agree closely with the experimental ones.[1,4]

The ratio max S/d_1 can be computed on the assumption that the sill acts like a low weir. The computed values, based on the papers of Forster and Skrinde[5] and of Kandaswamy and Rouse,[6] are compared to the experimental values in FIGURE 5. The results of the different investigators are surprisingly close, despite the fact that the sill in this study is similar to a broad-crested, nonaerated weir, while the sill of Forster and Skrinde is a sharp-crested, nonaerated weir, and the sill of Kandaswamy and Rouse is sharp-crested and aerated. Such a close agreement warrants the extrapolation in FIGURE 5.

Some of the experimental ratios result in numerical constants. Of these

$$\text{max } L_s/L = 1.25 \tag{2}$$

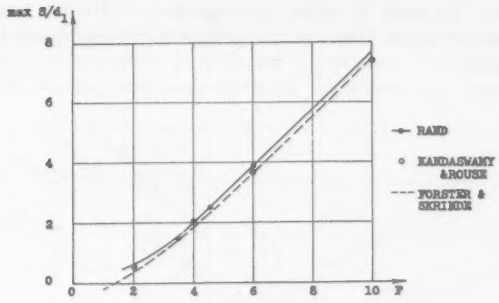

FIGURE 5.

is based on the values of L as given by the United States Bureau of Reclamation.[4] The distance max $L_s - L = 0.25\ L$ is needed to establish the depth d_2 upstream of the sill (FIGURES 4i and j). The other numerical constant ratio is

$$\min L_s/d_1 = 5.4 \tag{3}$$

independent of the Froude number over the experimental range.

The ratios max L_s/d_1, L/d_1, and min L_s/d_1 are given in FIGURE 7. Any intermediate basin length can be defined by the equation

$$L_s/d_1 = \min L_s/d_1 + K(\max L_s/d_1 - \min L_s/d_1) \tag{4}$$

with $0 \leqslant K \leqslant 1$. The values of K, as related to L_s/d_1, are shown in FIGURES 3 and 7. The coefficient K is an index of flow pattern in the

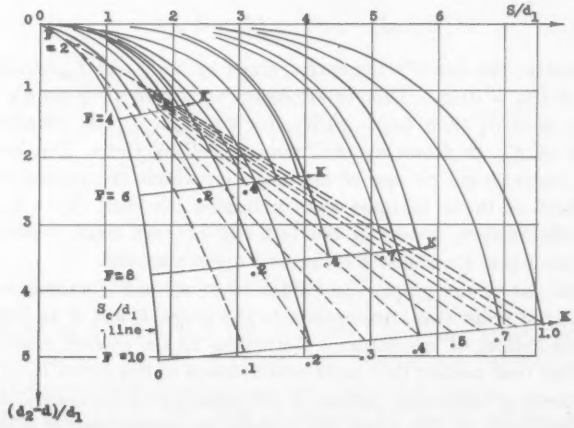

FIGURE 6.

stilling basin. To each K value corresponds a different range of flow geometry. The K values that are acceptable for design purposes will be determined later.

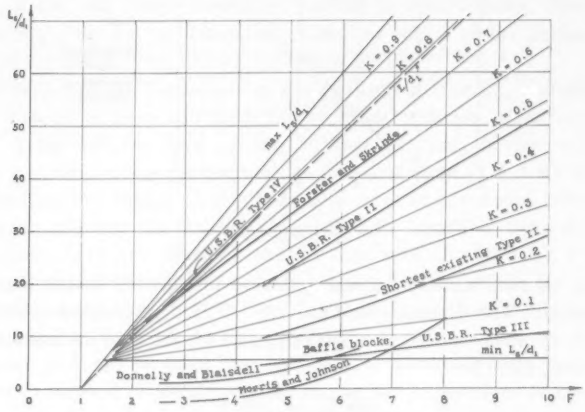

FIGURE 7.

The design of stilling basins is based upon the tailwater depth d that depends only on the hydraulic characteristics of the channel or river downstream of the basin. The range of d that affects the flow in the non-submerged stilling basin extends from d_j to d_2 (FIGURE 3). In this range, any d can be expressed by the dimensionless ratio $(d_2 - d)/d_1$, in the limits

$$0 \leqslant (d_2 - d)/d_1 \leqslant (d_2 - d_j)/d_1 \tag{5}$$

Consequently, the basin's geometry, given by S/d_1 and L_s/d_1, depends on F and $(d_2 - d)/d_1$. This relationship is shown in FIGURE 6. The variation of S/d_1 with $(d_2 - d)/d_1$, for constant L_s/d_1 (expressed by decimals of K), is drawn by full, curved, falling lines. The lower end of each curve stops on one of the axes on which the values of K are given. Each of these inclined axes indicates the limit $(d_2 - d_j)/d_1$ for the Froude number, shown at the left end of each axis. Dashed lines connect the equal K values at different Froude numbers.

For the use of the graph, the values of F, K, and d should be given. If $d > d_j$, the point that corresponds to the given F and K is determined (by interpolation, if necessary). Following in an upward direction the curved line that passes this point and belongs to the given F, or following the trend of adjoining curves, if interpolation is necessary, the S/d_1 that corresponds to the given $(d_2 - d)/d_1$ is determined. If $d < d_j$, the ratio d_j/d_1 should be used to determine S/d_1.

It is still possible that the use of FIGURE 6 may seem cumbersome due to needed interpolation in different directions. For clarity and possible use for design, FIGURES 8 to 12 are drawn for constant L_s/d_1

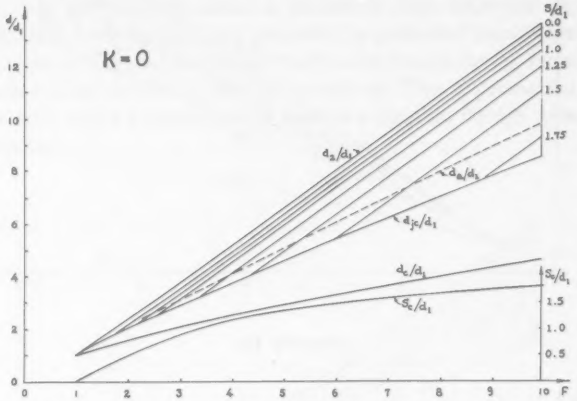

FIGURE 8.

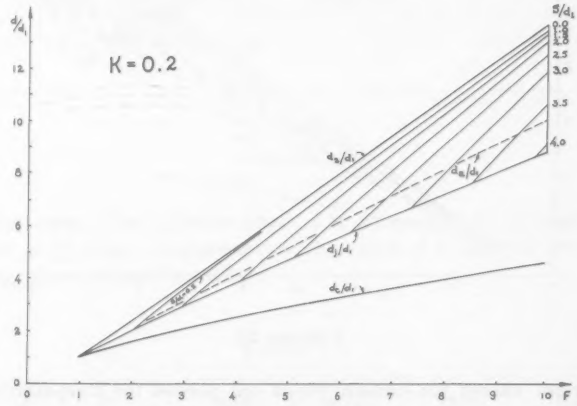

FIGURE 9.

values, given by $K = 0.0$; 0.2; 0.4; 0.7; and 1.0. These graphs are self-explanatory. With FIGURES 6 to 12, the relationship between all the dimensionless variables (see EQUATION 1) is established.

The basic values d_1, V_1, and d, needed for design of any stilling basin, can be determined by the known methods.[1,4] The tailwater depth d is often determined by direct measurements.

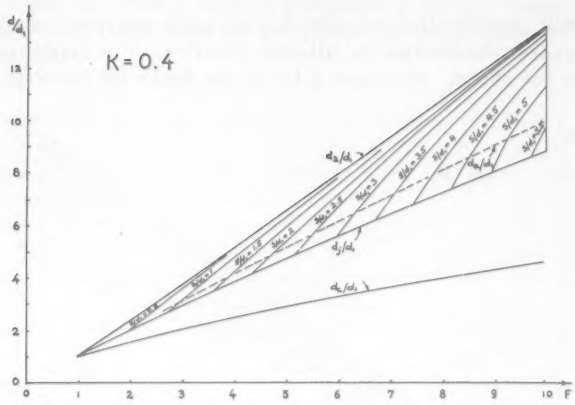

FIGURE 10.

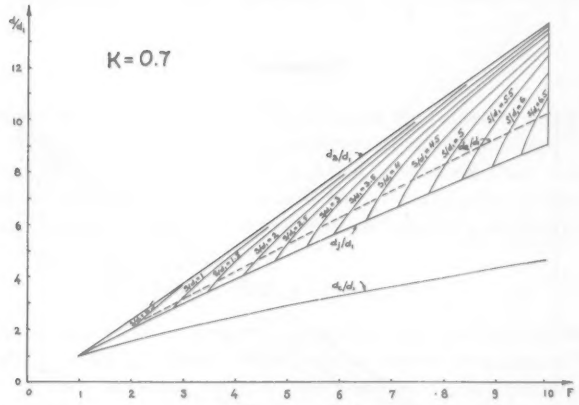

FIGURE 11.

Numerical values for certain ratios are needed for comparative computations in the latter part of the paper. These values are collected in TABLE 1. The drop number $D = q^2/gh^3$ in this table is a dimensionless similarity ratio for straight drop spillways.[1] The height h and the drop length L_d are defined in FIGURE 2.

Recommendations for Design

Two important points resulting from this study are: (1) a hydraulic jump of effective energy dissipation occurs in a stilling basin for a wide

range of related L_s and S values; and (2) this jump can be produced and kept in the basic position without any action of the tailwater. Consequently, the primary purpose of the tailwater is to provide a water cushion to protect the channel floor downstream of the sill against any harmful erosion. For erodable channels a relatively high tailwater is needed. However, if the channel is less erodable, or protected immediately downstream of the sill by an erosion-resistant cover (heavy riprap, for example), a tailwater much below d_2 may be permitted. The experimental results together with these considerations lead to a flexible design method, outlined below.

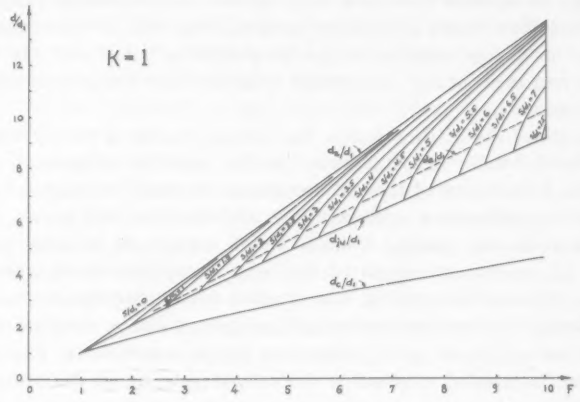

FIGURE 12.

The tailwater. The tailwater depth d is determined by hydraulic computations or by direct measurements. The value of d may fall into one of the three ranges defined hereafter.

TABLE 1

F	D	d_2/d_1	d_c/d_1	h/d_1	L_d/d_1
2	5.44	2.37	1.59	0.90	6.2
2.53	1	3.11	1.86	1.86	8.1
4	0.0351	5.19	2.52	7.70	13.5
6	0.00186	8.00	3.30	26.80	20.8
8	0.000235	10.82	4.00	64.80	28.1
10	0.0000615	13.65	4.64	118.00	35.5

If $d > d_2$, the entrance of the basin is submerged. The hydraulic jump would stay in the basic position even without a sill.

If d is in the range $d_2 \geqslant d \geqslant d_a$, with d_a greater than d_j and defined by

$$d_a = d_j + 0.25(d_2 - d_j) \tag{6}$$

a sill is needed to keep the jump in the basic position. The flow leaving the stilling basin is relatively calm, and the erosive action is limited (FIGURE 4h). The lower limit d_a for this depth range is determined from the observations of the flow pattern downstream of the sill. The factors in EQUATION 6 can be determined from FIGURE 6. The ratio d_a/d_1 is also given in FIGURES 8 to 12.

If $d < d_a$, an agitated flow with wavy surface, or a hydraulic jump, or a supercritical flow would occur downstream of the sill. If the channel is erodable, the design solution would be a stilling basin with the apron below the bed level of the downstream channel. This basin type will not be discussed.

The length of the stilling basin. The effectiveness of energy dissipation by the hydraulic jump increases as the basin is lengthened, while the erosion downstream of the sill decreases. However, a length close to max L_s is coupled with a relatively high sill that acts like a weir, and a stronger erosion may result. A basin length somewhere between min L_s and max L_s would correspond to the most acceptable erosion pattern. From the economic standpoint, the shortest acceptable basin should be recommended. Observations show that the limited energy dissipation and high exit velocities at min L_s make that length undesirable. FIGURE 3 shows that the needed increase in the tailwater depth, as the sill moves in downstream direction, is most pronounced over distances that correspond to a range of $K = 0.1$ to 0.2. At L_s that corresponds to $K = 0.2$, the hydraulic jump in the basin is very turbulent, and the flow at the sill is relatively tranquil, without any surface roller or waves. There is an effective bottom roller behind the sill that carries the channel material back toward the sill and helps to avoid erosion close to the stilling basin (FIGURE 4h). The studies of Heintz[7] on an erodable sand bed in the downstream channel show that the erosion pattern is acceptable at a basin length corresponding approximately to $K = 0.2$. On the basis of these considerations a shorter stilling basin than that given by $K = 0.2$ is not recommended, unless the channel is very resistant to erosion.

If there is much concern about the possible erosion, a longer stilling basin, corresponding to $K = 0.4$, could be adopted. In this case there is a highly turbulent jump in the basin, and the height of the sill is sufficient to insure the existence of a long effective bottom roller behind the sill.

A basin length that corresponds to K over 0.7 is normally not necessary. However, in the stilling basins that correspond to the Froude number smaller than 3, the hydraulic jump is wavy, unstable, and inef-

fective. A relatively long basin that corresponds to $K = 0.7$ to 0.9 is recommended for this situation.

The erosion downstream of the sill decreases generally with increasing tailwater depth. However, the basin's length should not be reduced merely because a relatively high tailwater exists. If the channel is very erodable, a longer stilling basin is needed even if the tailwater is high.

If $d > d_2$, a long surface roller may form in the basin, or the entrance flow may be lifted from the bottom (floating jet in the case of an overflow spillway). In both cases a lengthy turbulent region occurs, and a relatively long stilling basin with $K = 0.7$ or more may be needed.

This discussion shows that it is not advisable to fix the basin's length at some constant K value. A proper value of K for a given stilling basin can be chosen on the basis of the recommendations that are made above and will be supplemented by the analysis of the current design methods in the last part of this paper.

After the coefficient K has been determined, the basin's length L_s is found from FIGURE 7.

The height of the sill. After K has been chosen, the height of the sill S is determined from FIGURE 6, or from FIGURES 8 to 12. This height S will be sufficient to produce the hydraulic jump and keep it in the basic position for any given K and d. The highest sill at a given Froude number is the sill for $d = d_j$. For $d < d_j$ the hydraulic jump in the basin is independent of the tailwater, and no further increase of S is necessary. The addition of about 10 per cent to the determined value of S is recommended as a factor of safety, since the design discharge and its distribution in the basin's cross section cannot be determined with great accuracy.

For the tailwater depth d close to d_2 only a very low sill is theoretically needed. However, a certain sill height is required to insure the presence of a favorable bottom roller immediately downstream of the sill. An arbitrary value of minimum S equal to the critical height S_c of the sill, as defined in FIGURES 3, 4b and 4c, and plotted in FIGURES 6 and 8, is recommended. FIGURE 6 shows that S_c is two to four times smaller than the max S for $K = 1$, depending on the Froude number.

Analysis of Some of the Existing Design Methods

Most of the existing design methods offer a single solution for a given type of stilling basin. Even where these basins differ from the type used for this study, a comparison is still possible.

The stilling basins proposed by the United States Bureau of Reclamation[4] (USBR), Washington, D.C., will be analyzed first. The USBR Type II, with chute blocks and dentated vertical sill for $F = 4.5$, is somewhat similar to a stilling basin with a continuous vertical end sill. Assuming that the beginning of the horizontal apron coincides approximately with

the entrance to the stilling basin as defined in this paper, the ratio L_s/d_1 can be expressed in terms of the basin length L_{II} recommended by the USBR as follows

$$L_s/d_1 = d_2/d_1(L_{II}/d_2 - 0.42) \tag{7}$$

These L_s/d_1 values, together with the L_s/d_1 values for the shortest existing Type II basins, based on the data of the USBR, are plotted in FIGURE 7. The USBR L_{II} corresponds to $K = 0.4$ to $K = 0.5$, and is fully acceptable according to the present studies. However, the shortest existing Type II basins are clearly too short, as they correspond to K values of only 0.1 to 0.2.

The USBR does not recommend Type II basin for $F < 4.5$. FIGURE 7 shows the reason: K is decreasing with decreasing F, and not increasing as actually needed.

The sill height of Type II stilling basin will be analyzed next. The height of this sill, given by $S_{II}/d_2 = 0.2$, corresponds to

$$S_{II}/d_1 = 0.2\, d_2/d_1 \tag{8}$$

The corresponding tailwater depth is computed from FIGURE 6, as shown below, and then compared to the lowest d permitted by the USBR.

F	K	d_2/d_1 (from TABLE 1)	S_{II}/d_1	$(d_2-d)/d_1$ (FIGURE 6)	d/d_1	Lowest d/d_1 (USBR)
6	0.44	8.00	1.60	0.45	7.55	7.6
8	0.47	10.82	2.16	0.15	10.67	10.4
10	0.48	13.65	2.73	0.10	13.55	13.2

The agreement is close. According to these studies, the required tailwater depth is a little larger than that of the USBR for larger Froude numbers. However, both are close to d_2, as the sill height recommended by the USBR is relatively small.

To permit a lower tailwater, the Type III stilling basin is recommended by the USBR. This basin has baffle blocks at the midapron (0.8 d_2 from the entrance), and a relatively low end sill. The total length of this basin is close to the length of the shortest existing Type II basins. To analyze this basin, the baffle blocks are regarded as the end sill. The apron downstream of the baffle blocks then belongs to the downstream channel, being the local protection against the intense turbulence behind the sill. The basin's length is then equal to 0.8 d_2, or

$$L_s/d_1 = 0.8\, d_2/d_1 \tag{9}$$

FIGURE 7 shows that this basin length is equal to or a little more than min L_s. This is an acceptable length for a stilling basin with a protected channel floor just downstream of the sill, provided the tailwater depth d agrees with the height of the sill and is equal to or more than d_s

(EQUATION 6). The following computations, based on FIGURE 6, show that this is the case.

F	K	S/d_1 (blocks)	d_2/d_1	$(d_2-d)/d_1$ (FIGURE 6)	d/d_1	d_a/d_1 (EQUATION 6)	d/d_1 USBR
6	0.01	1.6	8.00	2.55	5.45	6.09	6.6
8	0.04	1.9	10.82	3.1	7.72	8.01	8.8
10	0.05	2.3	13.65	4.15	9.50	9.90	11.2

The results demonstrate that even a complicated stilling basin of Type III can be analyzed with the method described in this paper. Of course, the effect of the baffle blocks is not identical to that of a continuous sill, and a larger tailwater depth may be needed in the first case, as shown by these computations.

For Froude numbers $F = 2.5$ to 4.5 the USBR recommends the Type IV stilling basin, with the basin length equal to the length of the natural hydraulic jump ($K \sim 0.8$). The tailwater cannot be lowered below d_2 because of a very low sill that is optional. FIGURE 3 shows that, with a proper sill, a lower tailwater for $F = 4$ is possible.

Another interesting study on the length of stilling basins was conducted by Forster and Skrinde,[5] who used a sharp-crested sill that acted like a weir. The effect of the tailwater on the jump in the stilling basin was eliminated by using a low, supercritical tailwater. The resulting flow pattern was similar to that shown in FIGURE 4f. The basin length $L_s = 5\ d_2$ recommended by Forster and Skrinde corresponds to $K = 0.6$ to 0.7 (FIGURE 7). This excessive length results from the desire to provide an effective, turbulent jump in the stilling basin without having a high tailwater. The trend of variation of this basin length is sound, as it corresponds to larger K values for smaller F values. The basin length at $F = 2.5$ is equal to L, in agreement with the USBR Type IV stilling basin.

It is interesting to compare the height of the sharp-crested sill (FIGURE 6, curve $X/d_2 = 5$ in the paper of Forster and Skrinde) to the height of the rectangular sill for supercritical tailwater, and for the basin length that corresponds to $K = 0.7$ (FIGURE 11). The numerical values of S/d_1, given below, show the same trend and are of the same order of magnitude for both types of sill.

F	=	2	3	4	5	6
S/d_1 (Forster and Skrinde)	=	0.4	0.9	1.7	2.4	3.2
S/d_1 (Rand)	=	0.5	1.1	1.9	2.7	3.6

Since the present study is based on the experiments with straight drop spillway stilling basins, the analysis of existing design methods for these stilling basins must be relevant. The length of the horizontal apron of straight drop stilling basins consists of two parts (FIGURE 2). The drop length L_d determines the position of the entrance of the stilling basin,

and L_s is measured from this point in the downstream direction. The total length of the apron is then

$$L_{sd} = L_d + L_s \tag{10}$$

The drop length L_d as a function of the drop number D was determined in another paper.[1] As an approximation, L_d is given by

$$L_d/d_1 = 2.6\, d_2/d_1 \tag{11}$$

After L_s is determined from FIGURE 7 for an appropriate K, the total length of the apron for a straight drop stilling basin L_{sd} can be computed. Some of the existing design methods will be analyzed below.

The method of Donnelly and Blaisdell[8] proposes a total length of a straight drop stilling basin equal to

$$L_{sd} = x_a + 0.8\, d_c + 1.75\, d_c \tag{12}$$

where x_a determines the point at which the free falling nappe strikes the basin floor (approximated definition), $x_a + 0.8\, d_c$ determines the position of the bloor blocks, and $1.75\, d_c$ determines the distance between the floor blocks and the end of the basin, including the width of the end sill.

First, the position of the floor blocks is compared to L_d:

F	=	2.53	4	6
$(x_a + 0.8\, d_c)/d_1$	=	6.0	11.3	21.8
L_d/d_1	=	8.1	13.5	20.8

It follows that the floor blocks are placed close to the point where the actual flow on the apron starts, that is, practically at the entrance of the stilling basin. In fact, according to page 10 of Donnelly and Blaisdell's paper, it is necessary to "...permit the stream to become approximately parallel to the floor before it reaches the blocks."

As the main shock of the nappe is taken by the floor blocks, the stilling basin can be made relatively short. The basin length L_s can be computed as

$$L_s/d_1 = (x_a + 0.8\, d_c + 1.75\, d_c)/d_1 - L_d/d_1 \tag{13}$$

The results are

F	=	2.53	4	6
L_s/d_1	=	1.2	2.2	6.8

These L_s/d_1 values are even smaller than min L_s/d_1 (FIGURE 7). Such extreme shortness of the stilling basin is, of course, possible only because of the presence of the floor blocks. However, the USBR Type III stilling basin with floor blocks is relatively longer. Due to such shortness of the basin, the turbulence downstream of the end sill is certainly rather high, and a deep tailwater is needed to dampen it. This is in

fact the case. The minimum tailwater depth recommended by Donnelly and Blaisdell is $d = 2.15\ d_c$. A comparison of this d to the tailwater depth d_2 needed for a natural hydraulic jump shows that the needed tailwater depth d is above d_2 for very short stilling basins at smaller Froude numbers.

F	$d/d_1 = 2.15\ d_c/d_1$		d_2/d_1
2.53	4	>	3.11
4	5.4	>	5.19
6	7.1	<	8.0

This corresponds to a submerged toe of the nappe. At $F = 6$ a somewhat lower tailwater is permissible. Even when the tailwater is high, the relative shortness of the stilling basin proposed by Donnelly and Blaisdell should be accompanied by a reasonably high end sill. This is really the case. If their end sill ($S = 0.4\ d_c$) is compared to the highest sills used for min L_s in this study (S_c in FIGURES 6 and 8), the agreement is striking:

F	$S/d_1 = 0.4\ d_c/d_1$	S_c/d_1 (FIGURES 6 and 8)
2.53	0.75	0.75
4	1.01	1.17
6	1.32	1.50
8	1.60	1.65

According to my studies, a somewhat longer stilling basin for a straight drop spillway would result (EQUATION 10). This is comparable to the chute spillway stilling basin, and offers a larger possible variation in the allowable tailwater depth.

The stilling basin recommended by Morris and Johnson[9] will be the last one analyzed. Their three-dimensional drop model with an abrupt rise of the bottom at the end of the stilling basin, instead of an end sill, is different from the model used in this study. Nevertheless, the following observations can be made.

The height of the abrupt rise $S/d_1 = 0.5\ d_c/d_1$ is somewhat larger than S_c/d_1 in this study (FIGURE 8). Consequently, their stilling basin should not be much longer than min L_s. The total length of the stilling basin of Morris and Johnson, given in the symbols of the present paper, is

$$L_{sd}/d_1 = C_L \sqrt{(h/d_1 - 0.5\ d_c/d_1)\ d_c/d_1} \qquad (14)$$

Introducing the values of h/d_1, d_c/d_1, and L_d/d_1 from TABLE 1, and C_L from the graph in page 40 of the paper of Morris and Johnson, the ratio L_s/d_1 is computed from

$$L_s/d_1 = L_{sd}/d_1 - L_d/d_1 \qquad (15)$$

F =	2.53	4	6	8
L_s/d_1 =	−2.5	−1.8	3.3	13.1

These values, shown also in FIGURE 7, correspond to the shortest stilling basin imaginable. The abrupt rise is placed practically at the foot of the nappe, at the place where the floor blocks have been placed by Donnelly and Blaisdell. However, there is no extension of the basin behind the abrupt rise and, even if the basin is sufficient to produce a hydraulic jump, a rather pronounced erosion downstream of the basin may occur.

The analysis of various design methods shows that the straight drop stilling basins are comparatively much shorter than the chute spillway stilling basins. This may result from their unrelated experimental background, and may also reflect the effect of the individual judgment of different investigators. This analysis also demonstrates the extent of the range of acceptable solutions for the length of stilling basins.

Conclusions

The flow in the various types of stilling basins corresponds essentially to the flow in a simple basin, consisting of a level apron, and a continuous, vertical end sill. This flow makes it possible to analyze and compare different currently accepted design methods. This may lead to the formulation of a general method whereby structurally different basins may be designed, using a single set of basic principles.

In the present state, this method may serve as a guide for model studies and design of stilling basins.

Glossary

d	tailwater depth
d_1	the depth of supercritical flow at the entrance of the stilling basin
d_2	the tailwater depth for the natural hydraulic jump in the basic position and $S = 0$
d_c	the critical depth
d_j	the lowest tailwater depth that still affects the position of the hydraulic jump in the stilling basin
d_{jc}	d_j at min L_s and S_c
d_{jw}	d_j at max L_s and max S
d_s	smallest allowable tailwater depth for erodable downstream channel
D	drop number $= q^2/gh^3$
F	Froude number at the entrance of the stilling basin
g	acceleration of gravity
h	vertical height of a drop structure
K	coefficient to determine the basin length L_s
L	the length of the natural hydraulic jump
L_d	the drop length (FIGURE 2)

L_s the length of the stilling basin

L_{sd} the total length of apron of straight drop stilling basins

min L_s the shortest possible L_s without submerging the entrance of the stilling basin

max L_s the basin length that corresponds to $K = 1$

q discharge per unit width of the stilling basin

S the height of the sill

S_c the height of the sill at min L_s and d_{jc}

max S the highest sill that can be used without submerging the entrance of the stilling basin

V_1 the supercritical velocity at the entrance of the stilling basin

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SECTION OF PSYCHOLOGY

SOME PHYLOGENETIC ROOTS OF HUMAN BEHAVIOR*

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The thesis is presented that there is only a limited number of modes of dealing with danger available to the individual that have proved of general applicability exploitable in phylogeny. With respect to stimuli arising from within the organism, survival is favored by expulsion or by isolation. With respect to stimuli referring to a source of (potential) danger arising from the outer environment, survival is favored by flight, fight, submission, reversal, and vocalization, in the order of apparent phylogenetic development.

The modes of dealing with an internal danger appear to have persisted from their origins in protozoa. The nervous system's functions were drawn into the service of already established activities. With the long circuiting afforded by telencephalization, explicit action could be alternately expressed as implicit behavior at various levels of abstraction. This action permitted expulsion of internal danger not only physically but also psychically, as by repression of painful ideas, or by projection of previously introjected psychic objects. The physical isolation or walling-off of early phyla is encountered psychically as a defense mechanism of the ego. These phenomena have been studied by psychoanalysts [1, 2] whose explorations have at times entered upon the more biological fields, although there is yet an unbridged gap, to the filling of which this paper is offered as a contribution.

Philosophers, among them many psychoanalysts, have devoted much study to the question of whether there is some primary vital need, such as a tendency to increase a degree of internal order, to which all vital effort tends. It is supposed, then, that if there be some such definable principle, any threat to its fulfillment must represent a most basic threat and the source of the deepest, and least conscious, anxiety. Of many relevant [3-6] propositions, an especially interesting one by Gert Heilbrunn [7] states that the most basic, urgent fear of human beings is that of oral, passive destruction. "It is certain," wrote Heilbrunn "that most, if not all animals experience anxiety...it appears reasonable (on the basis of evidence both from the biological and from the psychoanalytic evidence) to speculate that their anxiety stems from the dread of annihilation by being eaten" and, I might add, especially by one of their own species

Ethnological observations on phyla linking man to his prechordate ancestors are more spotty than even the existing species would necessitate.

*This paper was presented at a meeting of the Section on November 18, 1957.

Most of my data deal with teleost fish, particularly the cichlids. Teleost fish do not lie in the direct line of descent:[8] indeed, the *Cichlidae* are a specialized group within the order, but perhaps this only shows in sharp relief potentialities already laid down in *their*, and our, ancestors, which come to expression in later phylogeny in infinite variety. It is among cichlids that we see most easily and in beautiful detail the ontogeny of our modes of defense against an external danger, and this offers full support to Heilbrunn's proposition.

When the young spawn of a cichlid clutch are assembled by their parents, before the yolk sac is absorbed, they lie randomly oriented. As the yolk sac becomes absorbed and they become free swimming, they remain more or less together in a school. Let there be a sudden movement of the water, as from a tap on one end of the tank or, as happens in nature, from the inrush of a hungry predator, and there is an extremely rapid flight. The school remains together if the stimulus comes from a distance; it disperses if the stimulus appears suddenly in their midst.

This flight permits group survival, though at great individual expense. Phylogenetically, it may long antedate the earliest protochordate but, once established, it has never been wholly abandoned, as we shall see, possibly because it has lent itself to mutational and environmental adaptations. Is our gregariousness a schooling tendency? In human behavioral terms we see in flight and schooling a prototype of the mass exodus from a city under enemy attack. When danger arises in our midst from a criminal we, too, disperse. Are the neurophysiological capacities for behavior so well laid down in the earliest vertebrates that, when men act with unreasoning terror in the face of sudden catastrophe, they must perforce act as did their protochordate ancestors?

In this schooling, of course, one member finds itself often enough behind and to the side of one or more of its fellows, perhaps on a slightly different plane. By the time this has been going on for from eight to ten weeks one notices that the position itself, associated perhaps with a movement that narrows the gap between the two members, initiates a flight of the leading member and a chase, rather than mere following, of the other. In the young fish, this may terminate in butting, or perhaps nipping, acts that in older fish are parts of a fight that often ends in death of one of the pair. This development of fighting has many interesting variations; particularly, one may note that actual fighting is less frequent than is the assumption of postures that have all the characteristics of threat and bluff. Lorenz and Evans[9] have called attention to the fundamental similarity between two adult male *Betta splendens* (Siamese fighting fish) with their gill coverings spread out, circling one another, and the intimidation behavior of Tom Sawyer and Alfred Temple. This resemblance is more than superficial. The specific evocative signals differ: Tom Sawyer would not bristle at sight of *Betta splendens*. The total body response differs in many ways, yet there are underlying constancies in

motor innervation (depression of the branchiostegial membrane in one, increased tension of the platysma and clenching of the jaws in the other; color display under influence of autonomic innervation in both) that convey a universal meaning, not only within, but also between vertebrate species.[10]

Cichlids express a continually increasing variety of patterns of aggressive behavior, much of it clearly related to attacks on food; naturally, therefore, on small moving objects. Under unfavorable circumstances this may cause the young to be eaten. Attacks on one another are increased after feeding.[11] Perhaps the conjunction of these two capacities is related to the tendency of mammalian mothers who eat the placenta or lick the young, to continue by eating the young.[12] Heilbrunn made this point and illustrated by references to psychoanalytic data some of the ways in which it is manifested in man.

Cichlids exhibit territorial possessiveness as well as various forms of hierarchical order based on aggressiveness.[13] To consider but one illustrative example, let two adult fish be introduced into a tank 12 x 18 x 18 inches, the second several days after the first one, so that the latter has time to establish possession. As soon as the newcomer approaches the possessor, the latter erects his median fins, his color becomes intense, his gill covers are brought forward and laterally to produce a wider head-on appearance, and he moves toward the intruder. If the intruder does not retreat forthwith, he (or she) erects the median fins and darkens in color. There then ensues, usually, a circling about one another, sometimes with tail beating, sometimes with butting (in some species, particularly at the genital region), nipping and, in more extreme instances, mouth-mouth biting or, as has been observed, eye enucleation. Commonly, when one fish is attacked by others, the scales are eaten off. The fight may terminate in a number of different ways. When it is between males, one may be killed or the loser may flee, in which case it may be chased and harried until it is either killed or assumes the attitude of inferiorism: motionless, median and pelvic fins laid against the body, head up, color pale, usually in an "obscure" position, for instance in a corner of the tank near the tip (if this makes for a dark background the fish may remain dark). If the fish does not flee, but assumes the attitude of inferiorism, it is usually not further attacked; the possessor swims away and may dig up sand for a while. If the intruder is a female, all goes much as above if she is not ready to spawn; but if she is ready she avoids the thrusts and beatings of the male rather than flee, and her own display and counterattacks are less intense. At the point of submission she retains her reproductive markings, giving to the submission the air of "symbolic inferiorism."[14] This act may then introduce specific mating behavior.

As regards submission, it is clearly an inhibition of motor acts, stopped, frozen between fight and flight. It is a dynamic state, ready in an instant to give way to action; it is not physiological exhaustion. This is the very beginning of the "secondary process" of Freud.[15] It would seem that

without development of this capacity for submission, for long-lasting inhibition of action (is it in this connection that the telencephalon developed further?) no further progress in vertebrate psychology could have been possible. In man submission manifests itself in many ways: keeping one's thoughts to oneself, intellectual activity, daydreaming, possibly even sleep, accepting a rebuke, submission to fate, the learning of lessons, sublimation of instinctual drives, acceptance of authority, humility – even to catatonia.[16] Without it man could never achieve the position nor the movements of depression; there could be no mysticism and no civilization.

In the final act of reversal, nature again achieved a solution before the contemplation of which we can be but humbly amazed. As it is seen in fish, this reversal appears as simplicity itself, *as if* the meaning of patterns of signals changed from "danger" to "no danger, but love." Tinbergen[17, 18] and Marler,[19] among others, have written extensively on this as it appears in higher vertebrates, but it is not until we come to man that we have sufficiently extensive observations to describe the vicissitudes of reversal psychically. From what I have been able to gather, it seems that out of reversal arises the capacity to deny reality; consequences of this may be delusions and hallucinations, it is true, but it may also be a requirement for a love of one's fellow man and for self-sacrifice in many forms and for religion.[20] It is a prerequisite to Kierkegaard's "knight of infinite faith."[21] Reversal as a fundamental phenomenon in denial is required for manic states[22,23] as normal and pathological conditions, but it must also be required for much adult love. Indeed, it may seriously be asked whether adult love is ever a "primary" affect; in the sense, let us say, of an infant's devotion to an object that it still includes within its own narcissistic ego.

We come now to a mechanism of defense of an order and quality that has not even been adumbrated in any of the previous discussion, namely, vocalization. In everything I have discussed thus far, the individual has been alone, even when in a school, and it has been a case of devil take the hindmost. With vocalization and the cry that calls upon others there is a new factor in life. This factor can be traced somewhat in its early development to the formation of the inner ear and the lateral line organ of fish. Both respond to vibrations in the water: the ear, generally to vibrations up to about 2700 cycles per second; the lateral line organ, up to some 182 cycles per second.[24] In fish nature seems to have tried many variations on themes with these organs but, behaviorally, their interplay is manifest in that even in young fry there is flight from fast, and approach toward slow, frequencies.[13]

In later phylogeny the specific sounds made by the young evoke activities on the part of adults that are nourishing and protective. Other sounds, of the wounded, of alarm, of rage, also serve the ends of survival in the presence of danger. Obversely, in the position of submission, sound as

well as movement may be inhibited. The use of sounds and of silences in dealing with danger by man has been the subject of extensive psychoanalytic literature.[25, 26]

No effort is made to define the ego mechanisms of defense as described by Anna Freud[1] in exact reference to the defenses noted above. In the first place, of course, I do not anthropomorphize fish; rather, I see in man developments from more primitive states. One cannot define the movements of piano playing by reference to the motions of the pectoral fins of fish, but one can say that the development of a pectoral fin and its innervation were prerequisites for the ability to play a piano.

The question may be raised whether my whole thesis is a play on words, a confusion resting in part on imputing to a behavioral fragment a meaning that it does not have; in part, on attaching to the behavior a word that has meaning only in human context and, even here, to debatable validity (for example, submission, reversal). It may be argued that similar goals may be achieved by various means, and a mass of relevant observations may be brought forward.[27,28] It may be argued that semeiotic chaos is created by describing limited activities and assuming homologies with behavior in other phyla on the (at times presumed) apparent "purpose" or mode of goal reaching and, on the basis of this, to apply a series of designata that refer to abstractions indicating highly complex implicit and explicit behavior and then, to cap it, by still further meta-abstractions. Yet it is precisely this that I assert makes the best sense. In the first place, no effort is made here to define by what means the solution is achieved in a given species; in the second place, it is thought that the means must serve "ends" of deeper biological determination than the occasional devices adapted to their achievement.

We are familiar with the efforts of many biologists to determine by what steps and part functions simple units of behavior become integrated into the flow of living activity. We are familiar also with the ways in which neurophysiologists and scientists in peripheral fields have been approaching the problem of sensory perception, memory, and thought processes as abstractions related to neural events;[29-31] also how Schilder,[2] for instance, started the closure of the gap between the neurological event and the psychic. It is just in this closure that a "cause" becomes an "implication," that the physical becomes metaphysical. At the same time psychoanalysts in some recent neurophysiological work have started from the realm of psychic life and its linguistic and other abstractions, and they have shown how, in the very infinity of specific instances within any set of psychic events or perceptions, there are discernible universals, or generalizations, the study of which reaches down into the neurological and biological roots of our being.[32,33] In this conjunction all trace is lost of discontinuity between man and his ancestors. It is in this conjunction, too, that such words as "reversal" may be defined both as observational experiential referata and as psychical events, implications, and consequences.

Man has been too much impressed with the *embarras de richesses* provided by the mammalian brain. We might do well to emulate Hughlings Jackson, who begged to be permitted to study first of all minor manifestations of epilepsy as being perhaps simpler to comprehend. If fish brains can do so much and if, in so many ways, human beings "irrationally" act as if they no longer possessed the full use of their acquisitions, perhaps we should do well to return to our ancestors if we wish to learn why we act as we do.

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DIVISION OF MYCOLOGY

INFLUENCE OF THERMOPERIODS ON PRODUCTION OF THE SEXUAL STAGE OF THE FUNGUS *SCLEROTINIA TRIFOLIORUM* ERIK.*

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The influence of external stimuli on the differentiation of the sexual stage of an organism is well illustrated by *Sclerotinia trifoliorum.* This fungus produces apothecial initials, the precursor to the sexual fruit body, in abundance when subjected to diurnal thermoperiods. The response is so striking that the external environment of temperature cycles during the 24-hour period must be important to the organism.

Diurnal thermoperiods have not been used as extensively as they should have been, insofar as I have been able to determine.[1,2] However, it is known that organisms are subjected to such an environment in nature. Other factors of the environment such as light, radiation, and certain artificial aspects of temperature (for example, constant temperature) have received considerable attention from research workers.[3] The influence of environment has been restricted almost entirely to light and darkness studies on sexual activity, plumage coloration, metamorphosis, dormancy, and migration of various animals. In nature it must be admitted that temperature changes during the 24-hour period have been taking place, and always will. Such external environment changes of temperature are probably not so important to organisms that live submerged in water. Loomis[4] recently demonstrated the importance of carbon dioxide tension in the sexual differentiation of *Hydra.* Organisms living submerged in water would not be influenced so greatly by temperature because water does not change temperature rapidly.

Thermoperiodicity studies have been used extensively, but relatively recently in conjunction with photoperiodicity in flowering, growth, and fruiting in the green plant.[5,6] It is now known that the tomato, when grown at night temperatures of 17° C., reaches an optimum in length, weight, and fruit production. The African violet flowers appear most luxuriantly at day temperatures of 60° F. with night temperatures of 70° F. or slightly higher. F. W. Went has reviewed this literature thoroughly.

In the fungi, as in higher plants, climatic factors or a set of factors control growth and development. Most of the studies conducted on tem-

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perature and reproduction have been confined to constant temperatures for extended periods, generally of less than 6 months' duration, sometimes for years. For example, Groves and Bowerman[7] (1955) grew *Sclerotinia borealis* for 3 months at 5° C., then in the dark at 0° C. for 2 months. After spermatization, cultures were placed at 5° C. for 1 month and, finally, removed to the greenhouse at 10° C. with a fluctuation of + or -5° C. Several workers in this group of fungi have resorted to the greenhouse for the production of the fruiting stage. Coe[8] did this with *Sclerotinia trifoliorum* and *Sclerotinia sclerotiorum*. Drayton[9] produced the perfect stage of *Botrytis convoluta*, that is, *Sclerotinia convoluta*, using long periods of variable temperatures. Transferring cultures to greenhouses, where day and night temperatures fluctuate, is common practice for the production of apothecia and perithecia of the fungi. Results generally cannot be repeated and therefore are usually unreliable. By using controlled conditions it is possible definitely to say that apothecial initials from *Sclerotinia trifoliorum* will appear on a certain day after inoculation.

Materials and Methods

Sclerotinia trifoliorum is a so-called homothallic or self-fertile organism. It made no difference whether sclerotia from single spore isolates or mass isolates were used for inoculum. All cultures were grown on ground whole wheat (50 gm./l.) plus 1.5 per cent agar as a basic medium. This medium was fortified at various times by many different substances before it was discovered that thermoperiodicity was the stimulus. None of the various nutritional compounds that were tried proved to be satisfactory substitutes for thermoperiods. No apothecial initials were produced by any nutritional substitution. Organic media tried were prune, carrot, beet, corn, potato dextrose, ground stems or leaves of legumes, barley, egg albumin, wheat germ, and lentil. Many other substances were also added to agar; these included vitamins, amino acids, phenylthiourea, hormones (estradiol, estrone), minor elements, sodium bicarbonate, and various inorganic compounds. No apothecial initials were produced unless sclerotia were subjected to thermoperiods.

Sclerotinia is not a difficult organism to grow; even small sclerotia are produced on plain 1.8 per cent water agar. Various media produce different numbers of sclerotia per plate. Size varies considerably when media contain high carbohydrate.

Cultures sometimes were started from agar blocks on which the fungus mycelium was growing vigorously. Almost all cultures, however, were started from sclerotia. More uniform cultures were obtained by the mycelium agar block method. After transplanting inoculum to Petri dishes containing 25 ml. of agar medium, they were sealed with paper masking tape. This is not necessary, but it helps in handling cultures and prevents excessive drying out of the medium.

Cultures were transferred to constant-temperature incubators, 4 cubic feet in size. Temperatures were recorded by a Leeds & Northrup Micromax, which provides a continuous record of hourly temperatures.

Photoperiod experiments were conducted before thermoperiod methods were known. Culture plates were exposed to 1500-foot candles of fluorescent light for 4-, 6-, 8-, 10-, 12-, 16-hour light and dark periods at a constant temperature of 20° C. No fundaments were produced at this constant temperature. No experiments were performed using a combination of thermoperiod and photoperiod. However, this appears unwise because apothecial initials have been produced when cultures were kept in total darkness. This was accomplished by sealing Petri dish culture plates in tightly enclosed cans.

An experiment was conducted to rule out the influence of water. Sclerotia were placed on sinter glass funnels in sterile conditions and means were provided for flushing the sclerotia with sterile water. This process had no influence on apothecial initial production.

In order to produce apothecia from initials, light is necessary.[10] This was provided by diffused sunlight or by fluorescent light while cultures were held at 18° C. The wave length of light or the intensity necessary for maturing initials to apothecia was not determined. Future experiments can provide this information now that apothecial initials can be produced regularly.

Preincubation

It is necessary to preincubate cultures for two weeks prior to subjection to a thermoperiod. Cultures that were cycled at various temperatures immediately on inoculation never produced apothecial initials. A preincubation temperature of 24° C. for a period of 25 days was found to be best for apothecial initial production when a thermoperiod of 21° C. for 8 hours and 15° C. for 16 hours was used (TABLE 1). Other possible experiments on the influence of incubation temperature have not yet been tried. It is possible that certain preincubation temperatures require a thermoperiod different from 21° C. for 8 hours and 15° C. for 16 hours. It is assumed that sclerotia must mature before they can be stimulated to produce their apothecial initials. Preincubation at 4° C. for several months was not favorable to the production of apothecial initials. Preincubation at a temperature of 15° C. for 6 months to a year was productive when these cultures were cycled. Cultures kept at the constant temperatures of 4°, 7°, 15°, 21°, 24°, 27° C. for periods extending from 4 weeks to 6 months did not produce apothecial initials.

Thermoperiod Studies

It was known by observation that sclerotia matured successfully at room temperature (21° ±5° C.). Consequently, after plates were inocu-

TABLE 1

PREINCUBATION TEMPERATURES AND NUMBER OF DAYS REQUIRED AT THESE TEMPERATURES BEFORE SUBJECTION TO TEMPERATURES OF 21° AND 15° C. IN THE 8- AND 16-HOUR PERIODS FOR THE PRODUCTION OF APOTHECIAL INITIALS IN *SCLEROTINIA TRIFOLIORUM* ERIK.

Preincubation (° C.)	*No. of days*	*No. of apothecial initials*						*No. of sclerotia*
		Mar. 9	*14*	*19*	*22*	*25*	*31*	
15	10	0	0	0	0	0	0	85
15	15	0	0	0	0	0	0	80
15	20	0	0	0	0	0	0	89
15	25	0	0	0	0	0	0	75
21	10	0	0	0	0	0	0	130
21	15	0	0	0	0	0	0	125
21	20	0	0	0	0	0	0	139
21	25	0	0	0	0	0	0	127
24	10	0	0	0	0	0	0	140
24	15	0	0	0	0	0	0	135
24	20	0	0	0	0	0	44	144
24	25	2	10	16	26	73	100	137
27	10	0	0	0	0	0	0	120
27	15	0	0	0	0	0	0	130
27	20	0	7	14	22	51	61	123
27	25	0	3	8	8	8	10	119
15	Continuous	0	0	0	0	0	0	85
21	"	0	0	0	0	0	0	130
24	"	0	0	0	0	0	0	136
27	"	0	0	0	0	0	0	124

lated, sclerotia were sealed with masking tape and left at room temperature for two weeks. They were then subjected to various thermoperiods (TABLE 2). Forty days from the time of inoculation, apothecial initials appeared, few in number at first and then in abundance. Sclerotia in these plates also varied in number, ranging from 179 per plate to 384. Some sclerotia never produced apothecial initials. However, some sclerotia in almost every plate did produce initials. The number of initials produced per sclerotium varied from 1 to 24. Three Petri dish cultures were used per temperature cycle as replications. All sclerotia could be regarded as replications, because all cultures originally were started from a single sclerotium. Size and number of sclerotia do not determine the number of apothecial initials that will be produced per plate.

The diurnal cycles used (TABLE 2) were of 8 hours and 16 hours; 4 hours and 20 hours. These were selected because of work done on green plants, and also because an inspection of soil temperature data indicated that these were the most logical.

Apothecial initials were produced in both diurnal cycles when certain temperatures were employed. The first apothecial initials to appear were in the thermoperiod of 8 hours at 21° C., 16 hours at 15° C. The greatest

TABLE 2

EFFECT OF CYCLING THERMOPERIODS ON THE PRODUCTION OF APOTHECIAL INITIALS IN *SCLEROTINIA TRIFOLIORUM* ERIK.

Incubation (° C.)					No. of apothecial initials			No. of sclerotia
4 hr.	8 hr.	16 hr.	20 hr.	24 hr.	13 days	21 days	27 days	
	7	15			0	0	31	202
	7	21			0	72	400	235
	7	24			0	2	34	190
	15	7			0	31	194	234
	15	21			0	7	48	290
	15	24			0	10	67	384
	21	7			0	2	35	179
	21	15			16	242	303	305
	21	24			0	0	0	255
	24	7			0	12	158	259
	24	15			1	48	245	186
	24	21			0	0	0	190
7			15		0	0	101	215
7			21		61	188	563	305
7			24		0	15	66	359
15			7		0	0	0	360
15			21		7	81	105	209
15			24		0	0	0	212
21			7		0	0	3	214
21			15		11	56	66	255
21			24		0	0	5	324
24			7		0	0	0	192
24			15		4	64	131	217
24			21		0	23	83	218
				7	0	0	0	226
				15	0	2	13	209
				21	0	0	34	232
				24	0	0	0	209

number in this diurnal cycle occurred at the thermoperiod of 8 hours at 7° C., 16 hours at 21° C. There were two thermoperiods that never produced apothecial initials – 8 hours at 21° C., 16 hours at 24° C.; 8 hours at 24° C., 16 hours at 21° C. Otherwise, all thermoperiods produced some apothecial initials. The diurnal cycle of 4 hours and 20 hours was also conducive to apothecial initial production. Here again the 7° and 21° C. thermoperiod produced the greatest number of initials. This same thermoperiod produced the earliest apothecial initials in quantity (61), 13 days from the beginning of the thermoperiod. At the end of 27 days, 563 initials were present on 305 sclerotia. This shows a tremendous stimulation in production of apothecial initials. Some sclerotia were so coated with initials that it was difficult to count them.

The data contained in TABLE 2 have been analyzed statistically by various methods. It is not possible to say that one diurnal cycle is more significant than the other. It is impossible to conclude that short cycles of low temperature, or long cycles of high temperature, or their opposites, are important. It is possible to say, however, that temperature cannot remain constant; temperature must vary during the diurnal cycle, and the variation from one period to another need not be extreme. Even a variation of 3° C. in the 4-hour and 20-hour cycle produced a few apothecial initials. It is possible that the cultures were not kept long enough. If readings had been taken after 27 days of cycling, more apothecial initials probably would have been produced. It is also possible that the ideal diurnal cycle and thermoperiod were not used. Preincubation temperatures might also influence a subsequent thermoperiod or diurnal cycle. A definite precise diumal rhythm might also be required. However, apothecial initials always appear when the cultures of *Sclerotinia trifoliorum* are cycled over a wide range of thermoperiods during two diurnal cycles.

It will now be possible to study the influence of thermoperiods on the initiation of the sexual stage of *Sclerotinia*. In this organism the light and dark periods are separated; therefore, the influence of light on the maturity of the apothecial initials to an apothecium can be studied in detail, histologically and chemically.

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*SECTION OF ANTHROPOLOGY**

MONTSERRAT, B. W. I.: SOME IMPLICATIONS OF SUSPENDED CULTURE CHANGE**

By Rhoda Métraux

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Culture change may be discussed from many points of view. In this paper I propose to discuss style rather than process of change. Insofar as one can observe, the culture that was studied – that of the peasants of Montserrat – is one in which change has taken place very slowly and in piecemeal fashion. However, as the study does not include a time dimension, that is, as it was carried out in one eleven-month period in 1953-1954, and as, in particular, our observations of individuals were limited to the period of the one study, any analysis of process is precluded. Today we are frequently confronted with the necessity of understanding change in cultures that have not previously been well studied. In such circumstances it has seemed to me that something can be gained through an analysis of the style of change as it can be defined in one moment of time. By style I mean the readiness to respond in ways that can be determined from the interrelationships of attitudes and beliefs and behavior in the culture.

In fact, we did not go to Montserrat to study any aspect of cultural change, but rather to study a problem connected with cultural imagery. The island was selected because it is small† and has a relatively small population of approximately 14,000 people.‡ Accordingly, I could hope to place the community studied in the island context. Montserrat was selected also because it had been a British colony since its first settlement in the Seventeenth Century, thus somewhat simplifying the problem of understanding its European and African heritages. Finally, I selected Montserrat because I had been assured that I should find there compact communities suitable for an intensive study of interaction. In fact, although Montserrat villages have a strong sense of identity, they are not compact. Danio Village,§ which we studied, consisted of about 500

*The Section of Oceanography and Meteorology held a meeting on November 26, 1957, at which William Eggert, United States Weather Bureau, Newark Airport, Newark, N. J., delivered a paper entitled "A New System of Weather Evaluation for the Aid of Approach-Pilots." This paper will appear in a later edition of the *Transactions*.

**This paper was presented at a meeting of the Section on November 25, 1957. The field work in Montserrat was done in collaboration with Theodora M. Abel, who spent one month in the field and worked primarily with projective tests.[1] The field work was supported by grants to the anthropologist by the Social Science Research Council, New York, N. Y., and to both field workers by the Institute for Intercultural Studies.

†In 1954, no survey map of the island existed; its size was stated to be between 32 and 47 square miles.[2]

‡Because of emigration, the island has a fluctuating population estimated in 1941 at 15,198, in 1948 at 13,700.

§"Danio Village" is a pseudonym.

people in 100 households clustered in several neighborhoods that I could encompass by rapid up- and downhill walking in about one hour's time.

In this paper I shall not attempt to discuss Montserrat social organization or culture as a whole. In the broadest sense, the island shares in the culture of the Caribbean area. It has long had a one-crop economy that shifted, at the end of the Nineteenth Century, from sugar to Sea Island cotton – an equally precarious crop on the world market. There are two principal social groups: on the one hand, estate owners, government officials, and merchants (today a mixed racial group) and, on the other hand, the peasantry, locally called "laborers," most of whom work as sharecroppers or renters or wage laborers on the estates and who also grow some subsistence crops; some of them may be small craftsmen or subordinate employees in town. The majority of the islanders are at least nominally Anglicans, though there is also a Roman Catholic minority. Most of the peasantry are semiliterate and an increasing number are better educated. The local need for skilled craftsmanship is a declining one, and few boys obtain or can make use of apprenticeship training; the usefulness of education to those who "work with the hoe" is minimal. Unlike most islands, Montserrat has a fluctuating, but only slowly growing population; migration drains off younger people, but also provides a major source of cash income to those who remain.

Although we did not intend to study change as such, I was made aware almost immediately upon my arrival that problems arising from a changing situation were a major preoccupation of Montserratians of all classes. For instance, relationships between estate owners and laborers had become so strained that a commission was brought to the island to attempt arbitration and to establish some mutual redefinition of responsibility for the island's welfare. It may be said that the country people were as critical of the labor union leaders as of members of the Cotton Growers' Association and that, indeed, they doubted both the usefulness of a labor union and their own ability to organize for any purposeful action.* In my initial contacts with the people of Danio Village, I was given continual small clues to their preoccupation with problems of change and to related attitudes: from their remarks that life in Montserrat, as compared to other places, was "dead" and that it was "hard, hard, hard"; from the scorn with which they spoke of the past and, individually, of those who still maintained beliefs from "before-time"; from the belief that good and useful people seldom came to the island and, having come, soon left again; from the angry contention that "they" (the government, estate owners, and townspeople) preferred foregoing improvement to letting the "black people" achieve anything; from the sulky discontent with which villagers viewed any improvement – a telephone system, a clinic, a

*In Danio Village the only active member of the labor union was the village entrepreneur, an extreme individualist who felt that the hands of all men were turned against him.

school – established in any community but their own; from their interpretations of the reasons for our coming and from the daydreams of what our stay might mean.

So pervasive was this thinking about change – its desirability, its possible effects, the unlikelihood of its occurrence – that I found it imperative to look to the past for a possible base line for working on the present situation. My inability to establish such a base line gave me my first insight into the handling of time – past, present, and future – and so into the style of change.

Whatever level one may have worked upon, the past shaded very rapidly into the mythological, and the mythological into the unknown; the past was something to be discarded, at best forgotten.* Consequently, for instance, there are few old buildings; when hurricanes destroyed houses, people did not rebuild but built anew, often in a slightly different spot. Local government records are very incomplete.† Danio Village formerly had been part of an estate; within the memory of living men and women it had grown into a village of freeholders after the estate owner had sold out his land and returned to England. Yet all memory of this man had shrunk, within 30 or 40 years, to the dimensions of a derisive rhyme. Even in the villagers' knowledge of each other's history there were gaps that were filled in only as some fact from the past took on contemporary significance, for instance in the interpretation of a quarrel – when everyone would suddenly "remember" that a woman's first-born child was not her husband's son and so, of course, the mother would resent the child's mistreatment by the social "father." The past contaminated the present in sickness and death. When a child became sick, one possible cause was the insistence of a dead person on being remembered and named through the child; when an adult died he might reappear as a "jumby" to plague those with whom he had unsettled troubles. Only rarely was the past invoked with pride or affection or nostalgia; on the contrary, "before-time" was when obeahmen (workers in magic) had been powerful and black magic was rampant; when hurricanes destroyed homes; when masters drove slaves to death; and when a son murdered his father in sudden anger.

However, there is one major exception to this negative attitude toward the past. Adults, watching children play or the dawdling of school children on the road, may grumble and scold, but they also envy them and recall with nostalgia the days of their own "idle, careless" childhood and youth. There is a real sense of an individual golden age of irresponsible, playful living that ends when a person "goes on his own

*Here one finds an important contrast to Haiti. Haitian peasants also have little concern with the historical past. However, a belief in an earlier golden age in social relationships provides them with an ideal framework for their interpretation of the present.[3]

†This statement is based on only a small sampling; in general, government records were not made available to the anthropologist. It is based also on hearsay, that is, on statements made about records by estate owners.

responsibility" and when a girl has a child. Even as people speak of the desirability of discipline, helpfulness and polite behavior, they criticize parents who restrict their children or make them work and become "hard."

If one now turns to attitudes toward the present and future, it is possible to deal with these in Montserrat terms only if one takes space as an aspect of time, for thinking about the future deeply involves ideas about the relationship of the island to the rest of the world. From one point of view, distant places are part of the community. Consequently, in attempting to define the limits of Danio Village, it was continually necessary to disregard boundaries in order to include absent community members – some long absent – in St. Kitts and Nevis and Antigua (other islands in the colony), or in Dominica and Trinidad, or in Curaçao and Aruba (in the Dutch West Indies), in Panama, or in New York, Boston, and Philadelphia, or in Toronto. Communication or lack of communication with the absent was an important part of the intravillage action as children and old people were (or were not) supported from abroad, as young women quarreled over the attentions of an absent lover, or as gossip flared into anger over conflicting news in letters sent and received. Equally important, there was continual coming and going between Montserrat and other islands. Actual experiences abroad, the tales of returned travelers, and speculations about those away from home combined to make Montserratians aware of contrasts between life on the island and elsewhere. Their feeling was that, as the world moved, they were left behind, were cheated out of use of labor-saving devices, and were condemned to work with their hands; that they were cheated out of the enjoyment of such luxuries as running water, electricity, bright lights, and the movies; that they were forced to remain dressed in home-made clothes while others were clad in the newest ready-made fashions; and that they grew lean while others fattened on enjoyment.* For the peasants the sea was not a road but a barrier lying simultaneously between places and between an undesired present and a desired future, a present that represented the past enduring at home and a future that was already the present for those fortunate enough to have left the island.

However, if the past had nightmare elements to be countered by scorn, detachment, and forgetfulness, distant places and the future had daydream elements built on inexperience and on interpretations of travelers' boasts. Therefore the future, like the past, edges off into the mythological. The chief bulwarks against the daydreams were, on the one hand, a belief in the greater friendliness and God-fearingness of people in Montserrat and, on the other, frank admission of fear of moving into a strange situation or even of crossing the sea; also there was an underlying love of the land of the island itself (although people did not see it as beauti-

*Balancing the distant places that represent the future, there are others, such as Haiti and the Dominican Republic, that still represent the past. To such islands it is said that people go to practice black magic against their enemies at home, now that knowledge of obeah has been more or less destroyed in Montserrat.

ful) and an expectation of disappointment as the usual outcome of experience.

In one important sphere of life there is a continuous attempt to discard the past and to bring the future home to the island; this is in religion – in the growing preference of Montserrat peasants for such evangelical sects as the Seventh Day Adventists, the Pentecostal Assemblies, and the Church of God of Prophecy. These churches are organized and run, with mission help from abroad, by the peasants themselves; consequently, they exercise in them a degree of autonomy otherwise denied them, and autonomy is perhaps the most highly valued trait for the individual. Through these churches, moreover, the peasants are provided with their only real models of how people organize and behave themselves elsewhere in the world. The emphasis in these evangelical churches in Montserrat is upon the brotherhood of man through Jesus (rather than upon the fatherhood of God), upon the imminence of Judgment, when all will be changed, not through the acts of men but through an act of God, and upon the need for disciplined behavior so as to be one of the participants in the coming glory. In practice – in the denial, for example, of the value of secular education and of the utility of medicine – there is a turning away from certain of the goals so important to Montserratians, so that, for those who are won to these sects, there is in the present a further stripping away of the meaningful content of life in an attempt to ensure a changed and better future.

If one now examines these conceptions of time and place, there emerges a picture of a people who are caught – and who are suspended as it were – between a discredited past and an ill-defined but desirable future in a present that is regarded as belonging to the past, a past that is hard and out of which one can move only by further detachment and discredit. There emerges a style of relationship to culture change in which detachment and autonomy are central, in which the emphasis is upon details of content in the desired goal rather than upon a way of life, and in which initiative, responsibility, and ideas of implementation are replaced by a belief in the value of altering one's location in a system (for example, by going abroad, by being "saved"). It is a style of relationship in which the imagination plays upon the possible with pleasure (but in which pleasure easily turns to boredom or to apathy, given no further cues), in which the new and the different arouse curiosity and are associated with the good and the useful (but in which curiosity easily turns to sour disappointment if there is no follow-up for oneself), and in which change is associated with success (but in which the success of others is a reproach to the self and their failure a source of self-justification).

One further point about time and autonomy must here be added. Along with the valuation of autonomy, there goes a real willingness to let individuals move at their own pace. Therefore, in work there is no great pressure for a group to accomplish a task at an even pace or in a given

time, or for an individual to match his pace with that of another. In the change of belief or practice there is also considerable tolerance for individual pace. Where one woman has gone to the hospital for twenty years to have her babies, another went only a year before to have her sixth child; where one family still "sets a table" for the dead on Christmas Eve, another has long since given up belief in the efficacy of the custom; "before-time" is the past in one's own definition and in terms of one's own experience. Even here, continuity, movement, and change are not determined solely by one's own actions but continually depend upon whether one is "spared" to meet again, to do something else, or to learn and act. Therefore, in the style of relationship to change there must be included a valuation of the individual's ability and pace as against a perception of the value of group interaction and movement in altering circumstances.

Observations of individuals in Montserrat suggest some implications for the individual of this style of relationship to culture change. For the adult – as also for the idle, carefree school child – there is a feeling that "you can't stop (hold) them." Given the feeling for autonomy, the individual moves or does not move, with little pressure to keep in step, but the individual can also depend on little support except through the choice of others. Consequently, there is little sense that support is likely to be steady. At the same time, with the rejection of the past and the unknown quality of the future – somewhere else, not here – there is a freedom to experiment and to play with possibilities and to improvise, but there is little to build on either in terms of a specific tradition handed down or of more general standards derived from traditions or in terms of goals toward which one can work with increased skills recognized to be part of the goals. So one sees in Montserrat intense interest in the new and a willingness to work very hard at a new kind of task. One also notices a treatment of the new and different as a fad or fashion that soon becomes boring and also a lack of differentiation between skilled and unskilled improvisation, between useful and useless (sometimes dangerous) experimentation. For example, a bitter argument arises about trying out a new fuel for lighting a lamp without reference to the type of lamp or the explosive properties of the fuel. The emphasis upon the fact that change takes place elsewhere and in the future, but not here and now, is a deterrent to responsible initiative in one's self and to recognition of it in others; accordingly it fosters, in some, a conflict between autonomy and dependence and, in others – those who are able to enjoy their lives, and there are some of these even in Montserrat – a sense that their very success is a kind of failure. It is not possible to say that Montserrat peasant culture ever had wholeness and integration, so one cannot determine whether its lack of integration was a predisposing factor to change or whether it has resulted from slow change, but one can say that in the present, given the mêlange of old and new and

the piecemeal fashion with which the new is brought in or is perceived, the individual must work exceptionally hard to gain extremely little in the way of a new integration. This in itself may set him apart from others rather than become a source of strength in his relationships to others.[4] That Montserratians are able to attempt adaptation to a mode of life rather than to bits and pieces is evident from the efforts of those who have embraced the beliefs of the evangelical sects,[5] but that this involves an impoverishment that others reject – arguing that "*all* things are good in moderation" – is evident from the willingness to accept the disappointing present or, alternatively, to break away from home and start anew elsewhere.

References

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3. MÉTRAUX, R. 1951. Kith and kin, a study of creole social structure in Marbial, Haiti. Unpublished dissertation. Columbia Univ. New York, N. Y.
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CONFERENCES HELD

SECTION OF BIOLOGY

CHLOROTHIAZIDE AND OTHER DIURETIC AGENTS

Friday, November 8, 1957

Conference Chairman: Robert F. Pitts
Cornell University Medical College,
New York, N. Y.

Session Chairman: John V. Taggart
College of Physicians and Surgeons, Columbia University,
New York, N. Y.

9:00 A.M.–
Greetings from the Academy – A. S. Gordon, Chairman, Section of Biology, The New York Academy of Sciences, New York, N. Y.; New York University, New York, N. Y.

"Renal Transport of Electrolytes" – Robert W. Berliner, National Heart Institute, Public Health Service, Bethesda, Md.

"Chemistry of Diuretic Agents" – James Sprague, Merck Sharp & Dohme Research Laboratories, West Point, Pa.

"Mechanism of Action of Mercurial and Xanthine Diuretics" – Gilbert Mudge, The Johns Hopkins University School of Medicine, Baltimore, Md.

"Mechanism of Action of Carbonic Anhydrase Inhibitors" – Alfred Gilman, The Albert Einstein College of Medicine, Yeshiva University, New York, N. Y.

"Mechanism of Action of Chlorothiazide" – Karl H. Beyer, Merck Sharp & Dohme Research Laboratories, West Point, Pa.

Open Discussion

12:30 P.M.–
Luncheon – Academy Building, 2 East 63rd Street, New York, N. Y.

Session Chairman: Homer W. Smith
New York University College of Medicine,
New York, N. Y.

2:00 P.M.–
"Disturbance in Fluid and Electrolyte Metabolism in Clinical Medicine" – Stanley Bradley, College of Physicians and Surgeons, Columbia University, New York, N. Y.

"Diuretic Therapy of Congestive Heart Failure" – Ralph Ford, Veterans Administration Hospital, Houston, Texas.

"Diuretic Therapy of Chronic Liver Disease" – John Laragh, College of Physicians and Surgeons, Columbia University, New York, N. Y.

"Diuretic Therapy of Chronic Renal Disease" – George Schreiner, Georgetown University School of Medicine, Washington, D. C.

Open Discussion

5:30 P.M. –
Cocktail Hour – Academy Building

6:30 P.M. –
Dinner – Academy Building

Session Chairman: Robert F. Pitts
Cornell University Medical College,
New York, N. Y.

8:00 P.M. –
"The Use of Chlorothiazide in the Treatment of Edema: A Comparison With Other Diuretic Agents" – Richard Bayliss, Westminister Hospital, London University, London, England.

Panel Discussion: "Antihypertensive Action of Chlorothiazide"

Edward Freis, Georgetown University Medical College, Washington, D. C.

John H. Moyer, Hahnemann Medical College and Hospital, Philadelphia, Pa.

Robert Wilkins, Massachusetts General Hospital, Boston, Mass.

SECTION OF PHYSICS AND CHEMISTRY

NUCLEAR MAGNETIC RESONANCE

Friday, November 22 and
Saturday, November 23, 1957

Conference Co-Chairmen

H. S. Gutowsky, University of Illinois,
Urbana, Ill.

F. C. Nachod, Sterling-Winthrop Research Institute,
Rensselaer, N. Y.

FRIDAY, NOVEMBER 22, 1957

9:00 A.M.–
Greetings from the Academy – Robert N. Boyd, Chairman, Section of Physics and Chemistry, The New York Academy of Sciences, New York, N. Y.; New York University, New York, N. Y.

Introduction to the Conference – F. C. Nachod, Sterling-Winthrop Research Institute, Rensselaer, N. Y.

"Nuclear Magnetic Resonance as a Phenomenon" – Charles P. Slichter, University of Illinois, Urbana, Ill.

"Requirements and Developments in Nuclear Magnetic Resonance Instrumentation" – Richard S. Sands, University of Michigan, Ann Arbor, Mich., and Varian Associates, Palo Alto, Calif.

"A High Stability Nuclear Magnetic Resonance Spectrometer" – Edward B. Baker, The Dow Chemical Company, Midland, Mich.

"Chemical Shifts and Electron Coupled Spin-Spin Interactions" – H. S. Gutowsky, University of Illinois, Urbana, Ill.

12:30 P.M.–
Luncheon – Academy Building, 2 East 63rd Street, New York, N. Y.

2:00 P.M.–
"Analysis of the Proton Magnetic Resonance Spectra of Pyridine" – H. J. Bernstein, National Research Council, Ottawa, Canada.

"Hindered Rotation in Organic Molecules" – W. D. Phillips, E. I. duPont de Nemours Company, Wilmington, Del.

"Organic Analytical Applications of Nuclear Magnetic Resonance Spectroscopy" – Aksel A. Bothner-By, Harvard University, Cambridge, Mass.

"Some Applications of C^{13} Nuclear Magnetic Resonance Spectra to Organic Chemistry" – Paul C. Lauterbur, Mellon Institute of Industrial Research, University of Pittsburgh, Pittsburgh, Pa.

"Proton Exchange Behaviors in Some Hydrogen Bonded Systems" – W. G. Schneider, National Research Council, Ottawa, Canada.

SATURDAY, NOVEMBER 23, 1957

9:00 A.M. –

"Nuclear Magnetic Resonance Spectra of Proteins" – Martin Saunders, Yale University, New Haven, Conn.

"Complementary Nuclear Magnetic Resonance and Infrared Absorption Spectroscopic Evaluations In Organo-Phosphorus Structural Chemistry" – Harold Finegold, National Bureau of Standards, Washington, D.C.

"Quantitative Determination of Organic Structures by Nuclear Magnetic Resonance" – R. B. Williams, Humble Oil and Refining Company, Baytown, Texas.

"Dipolar Interactions" – John S. Waugh, Massachusetts Institute of Technology, Cambridge, Mass.

"Nuclear Magnetic Resonance Studies in England" – George Murray, Pennsylvania State University, State College, Pa.

SECTION OF BIOLOGY

HODGKIN'S DISEASE

Monday, November 25 and
Tuesday, November 26, 1957

Conference Chairman: Antonio Rottino
St. Vincent's Hospital, New York, N. Y.

MONDAY, NOVEMBER 25, 1957

THE LYMPHOCYTE

Session Chairman: Albert S. Gordon
New York University, New York, N. Y.

9:00 A.M. –
Greetings from the Academy – Albert S. Gordon, Chairman, Section of Biology, The New York Academy of Sciences, New York, N. Y.; New York University, New York, N. Y.

"Outline of Problems in Hodgkin's Disease" – Antonio Rottino, St. Vincent's Hospital, New York, N. Y.

"Potentialities of the Lymphocyte" – John W. Rebuck, Henry Ford Hospital, Detroit, Mich.

"Control and Functions of the Lymphocyte" – Leonard Hamilton, Sloan-Kettering Institute for Cancer Research, New York, N. Y.

"Problems in Lymphocyte Production" – J. M. Yoffey, Bristol University, Bristol, England.

"Lymphocyte Metabolism" – Abraham White, Albert Einstein College of Medicine, Yeshiva University, New York, N. Y.

"Some Properties of Lymphocytes *in Vivo* and *in Vitro*" – O. A. Trowell, Medical Research Council, Radiobiological Research Unit, Harwell, England.

"The Thymic Lymphocytosis Stimulating Factor" – Donald Metcalf, Walter and Eliza Hall Institute of Medical Research, Royal Melbourne Hospital, Parkville, Australia.

12:30 P.M. –
Luncheon – Academy Building, 2 East 63rd Street, New York, N. Y.

THE RETICULUM CELL

Session Chairman: William E. Ehrich
University of Pennsylvania, Philadelphia, Pa.

2:00 P.M. –

"The Cytologic Identity and Interrelation of Mesenchymal Cells in Lymphoid Tissue" – Edward A. Gall, University of Cincinnati, Cincinnati, Ohio.

"An Electron Microscopic Study of Aspects of the Reticuloendothelial System" – Leon Weiss, Harvard Medical School, Boston, Mass.

"Some *in Vitro* Studies on Several Mesenchymal Cell Types Bearing on the Problem of the Reticuloendothelial System" – Kenneth M. Richter, University of Oklahoma, Norman, Okla.

"Studies on the Functional Interrelationship of Fibroblasts and Ground Substance Mucopolysaccharides" – R. D. Higginbotham, University of Utah, Salt Lake City, Utah.

"Observations on Morphology, Chemistry and Function of Mast Cells" – Earl P. Benditt, University of Washington, Seattle, Wash.

"Measurement of the Function of the Reticuloendothelium" – J. H. Heller, New England Institute for Medical Research, Ridgefield, Conn.

5:30 P.M. –

Cocktail Hour – Academy Building

TUESDAY, NOVEMBER 26, 1957

CYTOLOGY AND ETIOLOGY

Session Chairman: Antonio Rottino
St. Vincent's Hospital, New York, N.Y.

9:00 A.M. –

"Cytology of Sternberg Reed Cell as Revealed by the Electron Microscope" – Walter J. Frajola, Ohio State University, Columbus, Ohio.

"Cytologic Studies of the Nucleolus" – M. J. Kopac, New York University, New York, N.Y.

"Recent Advances in the Knowledge of the Eosinophile" – R. S. Speirs, State University of New York, Brooklyn, N.Y.

"Incidence of Hodgkin's Disease in the Army: Outcome of Armed Forces Cases" – Robert J. Lukes, Walter Reed Army Medical Center, Washington, D.C.

"Virus Etiology of Hodgkin's Disease" – Warren L. Bostick, University of California Medical Center, San Francisco, Calif.

"Etiologic Considerations in Hodgkin's Disease" – Robert Kassel, St. Vincent's Hospital, New York, N.Y.

"Use of Germ-Free Techniques in Search of Unknown Etiologic Agents" – James A. Reyniers, University of Notre Dame, Notre Dame, Ind.

12:30 P.M. –
Luncheon – Academy Building

THERAPY

Session Chairman: Lloyd F. Craver
Memorial Center for Cancer and Allied Diseases,
New York, N.Y.

2:00 P.M. –
"Results of Therapy in Hodgkin's Disease" – Henry Diamond, Memorial Center for Cancer and Allied Diseases, New York, N.Y.

"Megavoltage Therapy Using 2 MEV X Ray, with Five-Year Results" – Hugh F. Hare, Los Angeles Tumor Institute, Los Angeles, Calif.

"The Scope of Chemotherapy" – Sidney Farber, The Children's Cancer Research Foundation, Boston, Mass.

PANEL DISCUSSION

Chairman: Charles Doan
Ohio State University, Columbus, Ohio

and

Conference Speakers and Session Chairmen

NEW MEMBERS

Elected November 21, 1957

LIFE MEMBERSHIP

Smith, Burnett, Ph.D., Strategraphy. Retired, Skaneateles, N. Y.
Sterling, Edward C., Ph.D., Chemistry. Director, Research, Edcan Laboratories, South Norwalk, Conn.

SUSTAINING MEMBERSHIP

Steigmann, E. Albert, Ph.D., Photographic Chemical Work. Consulting Chemist, General Foods Corporation, Woburn, Mass.
Wodraska, Tibor W., M.D., Psychiatry. Supervising Psychiatrist, Rockland State Hospital, Orangeburg, N. Y.

ACTIVE MEMBERSHIP

Aginsky, Bernard W., Ph.D., Anthropology. Associate Professor, The City College of New York, New York, N. Y.
Aginsky, Ethel G., Ph.D., Anthropology. Associate Professor, Hunter College, New York, N. Y.
Aliminosa, Katharine V., B.A., Microbiology. Microbiologist, Brooklyn Botanic Gardens, Brooklyn, N. Y.
Auclair, Walter T., B.A., Cytochemistry. Graduate Student and Teaching Fellow, New York University, New York, N. Y.
Barron, John Mallett, D.D.S., Dentistry. Instructor, University of Tennessee College of Dentistry, Memphis, Tenn.
Bartone, John C., M.S., Anatomy. Student, St. Norbert College, West DePere, Wis.
Becker, Frederick Fenimore, M.D., Experimental Pathology. Research Fellow, New York University, New York, N. Y.
Biozzi, Guido, M.D., Immunology. Director, Research, Dentre National De La Recherche, Paris, France.
Brodkey, Jerald Steven, B.A., Physiology. Graduate Student, Physiology, National Institutes of Health Grant, Omaha, Nebr.
Burk, Barbara A., B.S., Psychology. Student, New York, N. Y.
Buzzati-Traverso, Adriano A., D.Nat.Sci., Genetics. Professor, University of Pavia, Pavia, Italy.
Cerletti, Aurelio, M.D., Pharmacology. Research Director, Sandoz Ltd., Basle, Switzerland.
Colonna, Frances I., M.D., Psychiatry. Psychiatrist, Lenox Hill Hospital, New York, N. Y.
Crossfield, Henry Charles, M.D., Cardiovascular Research. Associate, Princeton University, Princeton, N. J.
Crowell, Richard L., M.Sc., Virology. Graduate Student, University of Minnesota Medical School, Minneapolis, Minn.
DaVanzo, John P., Ph.D., Endocrinology. Fellow, Princeton University, Princeton, N. J.
Dellenback, Robert J., Ph.D., Physiology. Instructor, Columbia University, New York, N. Y.
De Merkline, Nicole, B.A., Biology. Student, New York, N. Y.
DeProspo, Nicholas D., Ph.D., Biology. Assistant Professor, Seton Hall University, South Orange, N. J.
Doko, Fumio, M.D., Biology. Fellow, Columbia University, New York, N. Y.
Engel, Andrew G., M.D., Cancer. Senior Assistant Surgeon, National Institutes of Health, Bethesda, Md.
Eylar, Ollie Roddy, Jr., M.S., Biology. Teaching and Research Assistant, University of Minnesota, Minneapolis, Minn.

Fahey, John L., M.D., Biology. Clinical Investigator, National Cancer Institute, Bethesda, Md.

Franklin, Stanley S., M.D., Cardiorenal Disease. Research Fellow, Peter Bent Brigham Hospital, Boston, Mass.

Funck-Bretano, Jean, M.D., Kidney. Director, Necker Hospital, Paris, France.

Gilbert, Ann C., M.A., Biology. Teacher, Midwood High School, Brooklyn, N. Y.

Gotsch, Ruth I., D.O., Internal Medicine. Practicing Physician, Watsonville, Calif.

Grinius, Alphonse F., M.D., Medical Research. United States Public Health, Fellow, Clark University, Worcester, Mass.

Guin, Grace Hughes, M.D., Pathology. Attending Pathologist, Children's Hospital, Washington, D. C.

Guinand-Baldo, Alberto, M.D., Cardiorenal Disease. Research Fellow, Medicine, Harvard Medical School, Boston, Mass.

Haidemenakis, Epimenides D., B.A., Physics. Research Physicist, New York, N. Y.

Hefferren, John J., Ph.D., Chemistry. Chemist, American Medical Association, Chicago, Ill.

Herzog, Hershel L., Ph.D., Organic Chemistry. Senior Research Chemist, Schering Corporation, Bloomfield, N. J.

Holland, John Joseph, Ph.D., Virology. Instructor, University of Minnesota Medical School, Minneapolis, Minn.

Houle, Edgar J. O., B.A., Chemistry. Student, New York, N. Y.

Isliker, Henry C., Ph.D., Biochemistry. Associate Biochemist, Medizinisch-chemisches Institut, Bern, Switzerland.

Jackson, Benjamin A., Ph.D., Cytology. Research Biologist, American Cyanamid Company, Pearl River, N. Y.

Jacobson, Moser Gamzu, M.S., Gas Analysis. Consulting Physicist, Mine Safety Appliance Company, Pittsburgh, Pa.

Joffe, Milton H., Ph.D., Toxicology. Physiologist, Chemical Warfare Laboratories, Army Chemical Center, Md.

Johnson, Hal G., Ph.D., Organic-Pharmaceutical Business. Vice-President, Vick Chemical Company, New York, N. Y.

Johnston, Christian, A.B., Polymer Physics. Belle Mead, N. J.

Jones, Clinton Eugene, M.S., Mathematics. Associate Professor, A. & I. State University, Nashville, Tenn.

Jordan, Robert Henry, Ph.D., Medicine. Associate Director, Clinical Research, Richmond, Va.

Judson, Charles M., Ph.D., Physical Chemistry. Section Manager, American Cyanamid Company, Stanford, Conn.

Kaley, Gabor, Pathology. Research Fellow, New York University College of Medicine, New York, N. Y.

Kaplan, Meyer, M.D., Medicine. Graduate Assistant, Louisiana State University School of Medicine, New Orleans, La.

Keeler, James H., Ph.D., Metallurgy. Liaison Scientist, General Electric Company, Schenectady, N. Y.

Keeler, Robert A., B.S., Chemistry. Group Leader, Vitro Laboratories, West Orange, N. J.

Kline, Ira, Ph.D., Cancer Chemotherapy. Department Head, Microbiological Association Inc., Bethesda, Md.

Kniazuk, Michael, B.E.E., Electronics. Manager, Technical Services, Merck Sharp & Dohme Research Laboratories, Rahway, N. J.

Knoepfler, Peter T., M.D., Psychiatry. Chief, Psychiatric Outpatient Department, U.S.A.F. Base Hospital, Sheppard AFB, Texas.

Kraut, Joseph, Ph.D., Protein Structure. Research Assistant Professor, University of Washington, Seattle, Wash.

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